One-Way Communication and Leading by Example:

Four Studies on Voluntary Cooperation in Public Goods Games

Dissertation

zur Erlangung des akademischen Grades

doctor rerum politicarum
(Dr. rer. pol.)

vorgelegt dem Rat der Wirtschaftswissenschaftlichen Fakultät der
Friedrich-Schiller-Universität Jena

am 25.01.2012

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Datum der Verteidigung: 23.04.2012

Deutsche Zusammenfassung

Die vorliegende Dissertation widmet sich mit Hilfe kontrollierter Laborexperimente der übergeordneten Frage, wie menschliche Kooperation in Situationen ermöglicht werden kann, in denen die materiellen Anreize egoistisches, nicht gruppendienliches Verhalten begünstigen. Diese Frage wird in der ökonomischen Literatur spätestens seit der Veröffentlichung von Samuelson (1954) zur Unterfunktion freier Märkte bei der Bereitstellung öffentlicher Güter und jener von Hardin (1968) zum Allmende-Problem an prominenter Stelle diskutiert. Sie ist allein schon aufgrund der Omnipräsenz von Kooperationsproblemen in Wirtschaft und Gesellschaft von höchster Relevanz. Beispiele, die das zentrale Problem, das soziale Dilemma, verdeutlichen, finden sich den internationalen Maßnahmen zum Klimaschutz, der Bereitstellung öffentlicher Parks, der Überfischung der Meere oder Trittbrettfahrerverhalten bei Teamarbeit.

Lösungsansätze für soziale Dilemmas unterscheiden sich maßgeblich darin, ob sie sich dem Menschenbild des Homo oeconomicus, des rational und gewinnmaximierend handelnden Agenten, verschreiben oder nicht (Kollock, 1998a). Die vorliegende Dissertation beschäftigt sich ausschließlich mit sogenannten motivationsbasierten Lösungsansätzen, welchen die Annahme zugrunde liegt, dass menschliches Handeln oftmals von anderen Argumenten als einzig dem eigenen monetären Gewinn geprägt ist. In der experimental-ökonomischen Literatur findet sich klare Evidenz für diese Annahme. Beispielsweise zeigen Experimente zu *Öffentlichen-Guts-Spielen*, dass etwa die Hälfte aller Teilnehmer das Verfolgen eigener Gewinnmaximierung zugunsten kooperativen Handelns zurückstellt, sofern andere Teilnehmer dieselbe Bereitschaft zeigen (Fischbacher et al., 2001, Fischbacher and Gächter, 2010). Aus der Perspektive solch "bedingt Kooperierender" können selbst Situationen, in denen der Homo oeconomicus unabhängig vom Verhalten anderer unkooperativ handeln würde, den Charakter von Koordinationsspielen besitzen, in denen sowohl gegenseitige Kooperation als auch das Gegenteil – gegenseitige Defektion – Verhaltensgleichgewichte darstellen (Rabin, 1993, Fehr and Schmidt, 2006).

Das einleitende Kapitel der vorliegenden Dissertation nimmt eine Einordnung in die Literatur vor und diskutiert grundsätzliche Überlegungen zu koope-

rativem Verhalten in sozialen Dilemmas. Anschließend werden vier Studien beschrieben, die jeweils als in sich geschlossene Kapitel verfasst sind. Alle vier Studien verwenden das *Öffentliche-Guts-Spiel* als experimentelles Paradigma, welches individuelle und kollektive Gewinnmaximierung scharf kontrastiert. Das *Öffentliche-Guts-Spiel* bildet eine Situation ab, in der jedes einzelne Mitglied einer Gruppe vor die Entscheidung gestellt ist, welchen Anteil seiner Anfangsausstattung es in ein gemeinsames Projekt einbringen will. Die Entscheidung, eine beliebige Einheit der Anfangsausstattung privat zu konsumieren (zu defektieren), ist dabei – unabhängig vom Handeln anderer Gruppenmitglieder – immer mit einem höheren individuellen materiellen Nutzen verbunden, als diese Einheit in das Gruppenprojekt einzubringen (zu kooperieren). Von positiven Beiträgen profitieren hingegen alle Mitglieder der Gruppe und zwar in einem Maße, das die individuellen Kosten des Beitragenden überkompensiert. In einem *Öffentlichen-Guts-Spiel* wäre die für den Homo oeconomicus dominierende Aktion, selbst nichts zum Projekt beizutragen. Aus der Perspektive des Kollektivs hingegen würde genau solches Verhalten zum geringstmöglichen materiellen Gewinn führen.

Kapitel 2 trägt den Titel "Leading by Words: A Voluntary Contribution Experiment with One-Way Communication". Es basiert auf einer gleichnamigen Studie, die in Zusammenarbeit mit Vittoria Levati und Anastasios Koukoumelis durchgeführt wurde und bei der Zeitschrift *Journal of Economic Behavior & Organization* zur Veröffentlichung akzeptiert ist. Darin untersuchen wir nicht-bindende Kommunikation als Lösungsansatz für erfolgreiches kollektives Handeln. Aus der Sicht rational und gewinnmaximierend handelnder Agenten kann so modellierte Kommunikation weder in einmaliger noch in endlich wiederholter Interaktion zu erhöhter Kooperation führen, da sie keine bindenden Zusagen für kooperatives Handeln erlaubt. In der experimentellen Verhaltensforschung ist die Wirksamkeit von Kommunikation jedoch unumstritten. Unklar bleibt trotz 50 Jahren intensiver Forschung aber, weshalb Kommunikation freiwillige Kooperation fördert. Der gegenwärtige Stand der Forschung besagt, dass Kommunikation zur Kooperation ermutigt, da sie ermöglicht, gegenseitige und glaubhafte Kooperations-Versprechen abzugeben. Diese Erklärung steht im Einklang mit der Sichtweise, dass Kommunikation bedingt Kooperierenden erlaubt, sich bezüglich bestimm-

ter Beiträge zum gemeinsamen Projekt zu koordinieren (Fehr and Fischbacher, 2002, Mohlin and Johannesson, 2008). Gegenseitige Versprechen sind aber keinesfalls das einzig denkbare Koordinationsinstrument. Deshalb stellt sich die Frage ihrer Notwendigkeit für kooperationsfördernde Kommunikation. Um diese Frage beantworten zu können, konzipierten wir ein Experiment, das Kommunikation ermöglicht, den gegenseitigen Austausch von Kooperations-Versprechen aber ausschließt. Dies wird erreicht, indem nur einseitige Kommunikation zulässig ist. Einseitige Kommunikation gestattet einem einzigen Mitglied einer Gruppe, eine schriftliche Nachricht an seine Mitspieler zu senden. Erst nachdem die Nachricht von allen Gruppenmitgliedern aufgenommen wurde, ist jedes einzelne dazu aufgefordert, eine bindende Entscheidung bezüglich seines Beitrags zu dem gemeinsamen Projekt zu treffen. Unsere Ergebnisse zeigen, dass Versuchsbedingungen mit einseitiger Kommunikation im Vergleich zu solchen ohne Kommunikation sowohl in einmaliger als auch in endlich wiederholter Interaktion zu signifikant erhöhter Kooperation führen. In endlich wiederholter Interaktion ist zudem kein Unterschied im Verhalten zu beobachten, wenn die Möglichkeit zur einseitigen Kommunikation entweder ausschließlich vor der ersten oder vor jeder Interaktion gegeben ist. Eine Analyse der Kommunikationsinhalte ergibt, dass die meisten Versender von Nachrichten genaue Vorschläge bezüglich der Beitragsentscheidung abgeben und dass diesen Vorschlägen überwiegend Folge geleistet wird. Unsere Ergebnisse erscheinen damit konsistent zu der Sichtweise, dass Kommunikation koordiniertes Verhalten ermöglicht. Sie zeigen allerdings auch, dass gegenseitige Versprechen nicht notwendig sind, um freiwillige Kooperation zu fördern.

Kapitel 3 trägt den Titel "A Voluntary Contribution Experiment with One-Way Communication and Endowment Asymmetry". Es basiert auf einer Studie, die in Zusammenarbeit mit Vittoria Levati und Anastasios Koukoumelis durchgeführt wurde und in der Zeitschrift *Applied Economics Letters* veröffentlicht werden wird. Darin erweitern wir die Untersuchung aus Kapitel 2, indem wir asymmetrische Anfangsausstattungen zulassen. In experimentellen Studien sind symmetrisch parametrisierte Entscheidungssituationen ein gängiger Ausgangspunkt. Asymmetrische Entscheidungssituationen außer Acht zu lassen, kann aber dazu führen, wichtige Merkmale menschlichen Entscheidungsverhaltens zu übersehen

(Güth et al., 2004). In Situationen, die kollektives Handeln erfordern, sind verschiedene Formen von Asymmetrie denkbar. Die von uns untersuchte Form asymmetrischer Anfangsausstattungen steht beispielsweise stellvertretend für Unterschiede bei der Verfügbarkeit von Mitteln für die Finanzierung öffentlicher Güter. Sie ist für unsere Studien von Interesse, da frühere Experimente zeigen, dass asymmetrische Anfangsausstattungen sowohl mit als auch ohne Kommunikationsmöglichkeiten zu geringerer Kooperation führen können (Isaac and Walker, 1988a, Cherry et al., 2005). Außerdem bedingt die effiziente Bereitstellung des öffentlichen Gutes bei asymmetrischen Anfangsausstattungen auch unterschiedlich hohe Beiträge der einzelnen Akteure. Die Wirksamkeit einseitiger Kommunikation könnte also vermindert sein, falls sich der Vorschlag, effizient beizutragen, also die komplette Anfangsausstattung einzusetzen, nicht durchsetzen lässt. Unsere Ergebnisse zeigen, dass einseitige Kommunikation in endlich wiederholter Interaktion auch bei asymmetrischen Anfangsausstattungen zu signifikant erhöhter und stabiler Kooperation führt. Ein wichtiges Merkmal unserer Daten ist, dass das Beitragsverhalten mit Bezug auf die Anfangsausstattung von der Versuchsbedingung abhängt. Ohne einseitige Kommunikation scheinen alle Teilnehmer einer unausgesprochenen Regel zu folgen, die gleiche Beiträge unabhängig von der Anfangsausstattung vorschreibt. Mit einseitiger Kommunikation hingegen entwickeln sich die Beiträge proportional zur Anfangsausstattung des jeweiligen Teilnehmers.

Die in Kapitel 4 vorgestellte Studie ist in Eigenarbeit entstanden und trägt den Titel “Leading by Words in Privileged Groups”. Hierin wird untersucht, ob sich einseitige Kommunikation auch dann positiv auf kooperatives Verhalten auswirkt, wenn Nachrichten keinen Bezug zur Entscheidungssituation aufweisen oder wenn Kooperation zu ungleichen Auszahlungen führt. Die erste Frage soll klären, ob einseitige Kommunikation allein deshalb wirksam ist, weil sie ein sozial günstigeres Entscheidungsumfeld schafft. Zur Klärung dieser Frage werden eine Kontrollbedingung ohne und zwei Versuchsbedingungen mit (einseitiger) Kommunikation verglichen. Von letzteren erlaubt nur eine Bedingung Nachrichten mit Bezug zum Experiment. Die Ergebnisse zeigen, dass nur experimentbezogene Nachrichten zu erhöhter Kooperation führen und legen deshalb nahe, dass der Mechanismus, welcher effektiver einseitiger Kommunikation unterliegt, im

Inhalt der Nachrichten begründet sein muss. Die diesbezüglich auffälligste Beobachtung aus den Kapiteln 2 und 3 ist, dass die Mehrzahl der Nachrichten spezifische Aufforderungen enthält, die komplette Anfangsausstattung beizutragen. Ein einheitliches Handeln gemäß solcher Aufforderungen hatte in den bisherigen Studien auch einheitliche Auszahlungen zur Folge. Diese Tatsache könnte wichtig für die Effektivität von einseitiger Kommunikation sein, falls Versuchspersonen eine Abneigung gegen ungleiche Auszahlungen hegen. Um zu untersuchen, ob einseitige Kommunikation (mit Bezug zum Experiment) auch dann zu erhöhter Kooperation führt, wenn daraus ungleiche Auszahlungen resultieren, wird die Effektivität von Kommunikation in normalen und in "privilegierten Gruppen" verglichen (Olson, 1965). Letztere unterscheiden sich von ersteren dadurch, dass eines der Gruppenmitglieder für jede bereitgestellte Einheit des öffentlichen Gutes ein zusätzliches Einkommen erhält. Als Folge führen einheitliche Beiträge in normalen Gruppen zu gleichen, in privilegierten Gruppen aber zu ungleichen Auszahlungen. Ein Vergleich zwischen beiden Arten von Gruppen ohne Kommunikationsmöglichkeiten ergibt, dass die Anwesenheit eines privilegierten Mitglieds zu geringeren Beiträgen aller anderen Gruppenmitglieder führt. Die Ergebnisse zeigen aber auch, dass einseitige Kommunikation selbst in privilegierten Gruppen zu erhöhter Kooperation führt, indem gerade jene Versuchsteilnehmer zur Kooperation ermutigt werden, die davon am wenigsten begünstigt sind.

Kapitel 5 basiert auf der in alleiniger Autorschaft durchgeführten Studie "Leading by Example in Intergroup Competition: An Experimental Approach". Diese Studie widmet sich der Frage, wie sich gute Beispiele auf kooperatives Verhalten auswirken, wenn sich zwei Gruppen in einem Konflikt gegenüberstehen. Die Auswirkung guter Beispiele wird durch den Vergleich von Beitragsverhalten zwischen Gruppen ermittelt, in denen die Entscheidungen entweder simultan oder semi-sequentiell getätigt werden. Semi-sequentiell bedeutet, dass der Beitrag eines Erstentscheiders allen übrigen Gruppenmitgliedern mitgeteilt wird, bevor diese ihre Beiträge als Zweitentscheider simultan und unabhängig voneinander bestimmen. Die Anwendung der Strategiemethode ermöglicht eine Charakterisierung der Zweitentscheider anhand ihrer Reaktionen auf unterschiedliche Beiträge des Erstentscheiders. Unsere Ergebnisse legen nahe, dass die in

der Literatur übliche Beobachtung, dass Zweitentscheider im Mittel weniger als Erstentscheider beitragen, auf typenspezifisches Verhalten zurückzuführen ist. Während sich viele Zweitentscheider reziprok zu Beiträgen von Erstentscheidern verhalten, sind auch reine Trittbrettfahrer und bedingungslos Beitragende zu beobachten. Ein nicht zu vernachlässigender Teil der Zweitentscheider bestraft gute Beispiele durch antireziprokes Verhalten sogar. Wie sich zeigt, hat die Präsenz eines Konflikts zwischen Gruppen differenzierte Auswirkungen. Erstentscheider bleiben unaffektiert. Zweitentscheider hingegen reagieren kooperativer auf Beiträge von Erstentscheidern. Bei mehrmaliger Interaktion verfehlen gute Beispiele in isoliert handelnden Gruppen ihre Wirkung. In Verbindung mit einem Konflikt zwischen Gruppen, führt beispielhaftes Verhalten hingegen zu erhöhter Kooperation.

Kapitel 6 der vorliegenden Arbeit beinhaltet eine Zusammenfassung sowie abschließende Überlegungen.

Acknowledgments

I take this opportunity to thank all those who supported me in my endeavor to prepare this dissertation.

First and foremost, I am greatly indebted to my supervisors in Jena, Prof. Oliver Kirchkamp and Prof. Werner Güth. I always found open doors and received patient and farsighted advice. Their profound knowledge, encouragement, and guidance helped me greatly in improving earlier versions of my dissertation in conception and in detail.

I thank my interdisciplinary advisor Prof. Christoph Engel for the open and fruitful discussions during each and every workshop. His skillful and insightful comments are valuable contributions to this thesis.

My deep gratitude goes to Prof. Vittoria Levati for her unconditional and constant support. Her devotion to science, her exemplary work attitude, and the hundreds of hours of open and constructive discussion with her were the best training I could possibly hope for. In short: working with her was a pleasure!

I am very grateful for the support of Prof. Gary Bornstein. He enabled my research stay at the Hebrew University in Jerusalem in difficult times and his help and advice made sure that it was a fruitful one.

The vital and stimulating environment at the Max Planck Institute of Economics set the perfect base for all my research efforts. The open doors and ears of all current and former ESI and IMPRS fellows, the vivid discussions and insightful comments greatly contributed to this thesis. In this respect, I am especially indebted to Ro'i Zultan, Fabian Winter, and Lauri Sääksvuori. I thank my dear office mates Sebastian Krügel and Matthias Uhl for their friendship, countless discussions (on- and off-topic), and the constructive but cheerful working atmosphere. The support of our coordinators Susanne Büchner and Tobias Uske was exceptional at all times. Without their help this dissertation would not have been possible. More generally, all the institute's staff, and in particular Silvia Arnold, Karin Richter, and Christoph Göring, have their share in this dissertation. Tatjana Puschkarsky was a huge support in proof-reading the introduction.

Without doubt, this thesis would not have been possible if it was not for the constant social support of my family and my girlfriend Katja. My parents' home

served as the safe harbor more than once – their advice and support was always assured. Both, my sister Anne and my brother Benjamin, helped to free my mind several times. And then, there is this certain someone to whom I owe my sanity, my emotional balance, and most of the nicest memories. Katja, this is all on you!

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CHAPTER 1

INTRODUCTION

> "Your corn is ripe today; mine will be so tomorrow. 'Tis profitable for us both, that I should labour with you today, and that you should aid me tomorrow. I have no kindness for you, and know you have as little for me. I will not, therefore, take any pains upon your account; and should I labour with you upon my own account, in expectation of a return, I know I should be disappointed, and that I should in vain depend upon your gratitude. Here then I leave you to labour alone; You treat me in the same manner. The seasons change; and both of us lose our harvests for want of mutual confidence and security."
>
> DAVID HUME, "TREATISE ON HUMAN UNDERSTANDING", 1739, BOOK III, PART II, SECTION V

1.1 Social Dilemmas

How to promote voluntary cooperation in situations such as the one described by Hume? This shall be the overarching question to whose answer this thesis attempts to contribute. The collective action problem that Hume's farmers face is intricate. What is to be gained collectively are the fruits of cooperation. Every farmer would be better off if both of them assisted in the harvest of the other. This is evident to both parties. However, as the narrating farmer notes, doing so is not in his own interest since he could not expect reciprocation for his help if he chose to assist on the first day. In the absence of binding commitment opportunities both farmers are trapped in an instance of what is called a social dilemma.[1]

[1] According to Dawes (1980), a social dilemma is characterized by two properties: (i) regardless of the behavior of other actors, an individual's monetary payoff for a non-cooperative choice (defection) is always greater than that for a cooperative choice, but (ii) everyone is worse off

The challenges posed by social dilemmas are as relevant today as they were at the times of the enlightenment (and throughout human history). One example where social dilemmas are apparent is in the provision of public goods such as climate stability, public parks, or public television, to name just a few. Public goods are non-rival in consumption, i.e., someone's use of a public good does not diminish its availability to anyone else, and non-excludable, i.e., no one can be excluded from their use (Samuelson, 1954).[2] The latter property implies that the consumption of the public good is free once it has been made available. Since contributing to the public good is costly, a rational and self-regarding decision maker prefers to spare the cost and to free ride on the contributions by others.[3] However, if everyone behaves in this way, the public good is not provided, and everybody is worse off. This point was made famous by Olson in his book "The Logic of Collective Action" in 1965. Another form of social dilemmas is exemplified in phenomena such as the overuse of irrigation water or the over-harvesting of natural resources such as fish or wood (for an overview, see Ostrom, 1990). The basic problem was already addressed by Aristotle ("Politics", Book II, Chapter 3) but is inseparably linked to the parabola of *the tragedy of the commons* (Hardin, 1968). Hardin describes a group of herders who all have access to the same commons. Assuming that herdsmen are rational and self-regarding, he concludes that every herder will prefer more of his own cattle grazing on the commons as the benefits from such actions accrue to the individual herder while the costs in form of grazed pasture are born by the collective. Since the capacity of a commons is finite, overgrazing and a depletion of the natural resource will be the consequence if all herders follow this approach. In contrast to public goods, the use of a commons is characterized by rivalry in consumption since, e.g., every fish can only be eaten once.

Due to its central role for the understanding of human cooperation, social

if all individuals choose to defect than if they all choose to cooperate. See Kollock (1998a) for a definition which does not impose that defection is a dominating choice.

[2] Pure public goods are truly non-rival and non-excludable. However, many public goods exhibit these two properties to varying degrees, for example in the form of significant costs associated with the exclusion of single individuals.

[3] The term self-regarding will be used throughout this thesis in order to describe behavior that is geared towards the maximization of own monetary benefits. It stands in contrast to other-regarding behavior which gives a strictly positive weight to the monetary outcomes of individuals other than the decision maker.

dilemmas figure prominently in empirical research of many social sciences such as psychology, political science, sociology, and economics (for overviews, see Dawes, 1980, Ostrom et al., 1994, Kollock, 1998a). Social dilemmas are typically studied using the commons dilemma and the public goods game as canonical (N>2 player) game form representations of the tragedy of the commons and the problem of public good provision. The third central model in this field is the prisoner's dilemma game. It corresponds to Hume's farmer example in that it involves two actors who face the binary decision between cooperation and defection, but is usually described as a game with simultaneous rather than sequential decisions.[4]

Extensive field work as well as laboratory experiments have immensely contributed to our understanding of how humans cope with social dilemma problems. Field studies, mostly investigating common pool resources, have generated important insights, for example with regard to the institutional and organizational arrangements which are conducive to the protection of natural resources (Ostrom, 1990).[5] A prominent theme in laboratory and field experiments is to identify ways that may facilitate cooperative behavior in collective action problems. Such solutions to social dilemmas differ in whether they imply changes to the basic structure of the decision situation and whether they are feasible for rational and self-regarding actors (Kollock, 1998a).

Structural solutions modify the basic decision situation in such a way that the social dilemma is alleviated or even entirely eliminated. Arguably the most radical such solution is to establish a central authority which manages the commons – an idea based on Hobbes' concept of the Leviathan. Hardin (1968) picks up on the same concept acknowledging that the necessary interventions in the freedom of the individual might be greatly unjust. Another commonly discussed topic is the reallocation of property rights. Like establishing a central authority (for an experiment, see, e.g., Samuelson and Messick, 1986), this approach assumes that individuals can be effectively excluded from the commons. Other structural solu-

[4]The simultaneous prisoner's dilemma game was developed in 1950 by Marrill Flood and Melvin Dresher – two scientists at RAND cooperation. It owes its name to Albert Tucker who invented the story of two accused criminals who, being interrogated individually, can choose to blame the other (defect) or remain silent (cooperate).

[5]Poteete et al. (2010) provide an extensive overview on the methods used in field research on the commons.

tions extend the decision situation to allow for rewards or punishment. Already Caldwell (1976), Komorita (1987), and Yamagishi (1986) observe that cooperation is more likely if defecting individuals may be sanctioned.[6] This result is far from being trivial since the activity of punishing is itself a (second order) public good.[7] Intergroup competition can also be considered to be a structural solution to intragroup cooperation problems since exogenous prices may be used to create incentives for cooperative acts (see, e.g., Bornstein et al., 1990).[8] Some other structural factors that are relevant for behavior include group size (Isaac and Walker, 1988a) and the marginal costs for cooperative choices (Marwell and Ames, 1979).

Strategic solutions to social dilemmas do not alter the structure of the game and are feasible to rational and self-regarding individuals. They are confined to repeated interaction since they rely on strategic behavior. Arguably most important in this respect are the computer tournaments designed by Axelrod (1984) which show, among other things, that a reciprocal strategy such as Tit-for-Tat may encourage cooperation among rational and self-regarding actors.

Motivational solutions to collective action problems do not modify the social dilemma situation and may only affect cooperation if (some) actors are motivated by concerns other than their own monetary outcomes. As social psychologists have known for a long time, elevating an individual's sense of group membership is such a motivational solution (see, e.g., Tajfel and Turner, 1986). This insight led to experiments which show that identifying with a group can lead to more restraint in a commons dilemma (Brewer and Kramer, 1986) or that exercises designed to foster group identification can yield more cooperative behavior in public goods games (Eckel and Grossman, 2005).[9] Introducing leadership is

[6] Following Fehr and Gächter (2000), this field of research also received a lot of attention in economics.

[7] Institutions that allow for sanctioning are often considered problematic as the benefits of increased cooperation usually fail to outweigh the costs inflicted by punishment. In this context, Sääksvuori et al. (2011) were the first to demonstrate that group competition may enhance the effectiveness of sanctioning.

[8] Bornstein and Ben-Yossef (1994) showed that intergroup competition has a second, purely motivational effect on intragroup cooperation. This distinction will be of importance for the study presented in Chapter 5.

[9] The mechanisms that drive the effects of group identification are still subject to controversial discussion (see, e.g., Simpson, 2006, Yamagishi and Mifune, 2008). The core of the dispute is whether group identification affects individuals' preferences, their beliefs, or both. For an excellent review on this discussion, see Guala et al. (2009). For economic studies on group identity, see, e.g., Akerlof and Kranton (2000) and Chen and Li (2009).

another motivational solution. It is distinct from formal authority in that leaders do not have the power to make decisions on behalf of other actors (Hermalin, 1998). In economics, leadership has mainly been studied in the form of leading by example, where the only means by which a leader can motivate cooperative behavior from others is his own exemplary effort. A continuing stream of experimental studies shows that leading by example can be conducive to cooperation (Moxnes and van der Heijden, 2003, Güth et al., 2007, Levati et al., 2007), especially when the act of leading is voluntary (Rivas and Sutter, 2011).

The most widely replicated result with respect to motivational solutions is that costless, non-binding, and non-verifiable communication has a positive effect on cooperation (for surveys of relevant work in psychology and economics, see Kopelman et al., 2002, Bicchieri and Lev-On, 2007, respectively). Despite the fact that this form of communication is nothing but "cheap talk," the behavioral effect of communication proves to be astonishingly robust. It has been demonstrated in commons, public goods and prisoner's dilemmas, for repeated as well as for one-shot interaction, and for various ways of transmitting the communication content, e.g. face-to-face or through electronic media.[10] While the effect of communication on the propensity to act collectively was already shown by Deutsch (1958), its underlying mechanisms are still subject to vivid discussion. At present, the two leading explanations are (i) that communication creates group identification and (ii) that communication facilitates the mutual exchange of promises to cooperate which are then reinforced by a social norm of promise-keeping (for a survey on the possible explanations, see Shankar and Pavitt, 2002). While the importance of promises is rarely questioned, several experimental results cast doubt on the explanation based on group identification.[11] Possibly most insightful in this respect is the finding that communication on a relevant topic other than the social dilemma proves to be insufficient for enhancing cooperation (Dawes et al., 1977, Bouas and Komorita, 1996). If communication itself fosters group identification which then increases cooperation, the contrary should be expected.[12]

[10] For a recent meta study on this topic see Balliet (2010).

[11] Already Kerr et al. (1997, p. 1301) notes that "[v]arious lines of evidence converge on the conclusion that such commitment-keeping underlies (or at least contributes to) the cooperation-enhancing effect of group discussion" (parentheses in the original).

[12] For a more comprehensive discussion of evidence in favor of an explanation based on promises, see Chapter 4 in Bicchieri (2006).

It is the aim of this thesis to expand the knowledge of human behavior in collective action problems. More specifically, we[13] are interested in non-coercive means to stimulate voluntary cooperative behavior. To this end, we investigate two motivational solutions to the free-rider problem in a series of studies that are organized in four self-contained chapters. Chapters 2 to 4 are dedicated to advance the current understanding of how communication affects behavior in social dilemmas. The key innovation presented in Chapter 2 is the introduction of one-way communication as a sparse mechanism that only allows for unidirectional written messages by a single member of a group. One-way communication precludes the mutual exchange of promises but is shown to increases cooperation. This results challenges the cherished view that mutual promises are necessary for communication to be effective. The studies presented in Chapters 3 and 4 investigate the effects of one-way communication in various asymmetric decision environments. The key observations are that one-way messages may stimulate contributions to public goods in proportion to asymmetric endowments, and that one-way communication enhances cooperation even if such behavior leads to inequality in earnings. In Chapter 5, we augment the knowledge on leading by example in social dilemmas as we (i) generalize the paradigm to a scenario of intergroup competition and (ii) categorize follower behavior based on conditional responses with respect to leaders' examples.

A common aspect to all studies presented in this thesis, is the application of laboratory experimental methods. More specifically, we make consistent use of the linear public goods game as an experimental paradigm that generates sharp contrast between the individuals' and the collective's monetary interests. In the public goods game neither communication nor leading by example can be expected to shape the behavior of (a homogenous population of) rational and self-regarding actors. Consequently, we will draw on economic theory that models other regarding motivations in order to generate hypotheses. In the following section, we will briefly introduce the structure of the linear public goods game, highlight important behavioral regularities, and discuss their consequences for possible solutions to the free-rider problem.

[13]The first person plural will be used throughout this thesis. This approach serves consistency purposes. It is also justified because many of the more general considerations in the introduction and the conclusion pertain to the single- and the co-authored studies alike.

1.2 Conditional Cooperation and Coordination in Public Goods Games

In its most common format, the linear public goods game was introduced by Isaac et al. (1984). Its fully symmetric version depicts a situation in which N>2 members of a group, each endowed with E tokens, decide simultaneously and independently about their contributions (each between 0 and E) to a common project, i.e., the public good. Every token a decision maker keeps is worth 1 token to him. Additionally, he earns β tokens for every token he or any other group member contributes to the public good. Denoting individual i's contribution by c_i, his payoff is given by

$$\pi_i(\mathbf{c}) \quad = \quad 25 - c_i + \beta \times \sum_{j=1}^{N} c_j \quad , \tag{1.1}$$

where $\mathbf{c} = (c_1, \ldots, c_N)$ and $\sum_{j=1}^{N} c_j$ represent the strategy profile and the sum of contribution in i's group, respectively. Usually, public goods games are parameterized such that $0 < \beta < 1$ and $N \times \beta > 1$. Due to the first inequality, contributing a token is associated with a net loss of $1 - \beta > 0$ tokens irrespective of the decisions of the other group members. A rational and self-regarding decision maker thus prefers to contribute nothing to the public good. However, due to the second inequality, it is efficient to contribute the full endowment in the sense that it maximizes the sum of material payoffs in a group.

By now, a large number of experiments has established various behavioral regularities that are commonly observed in public goods games (for surveys, see Ledyard, 1995, Chaudhuri, 2011). Particularly notable stylized facts are that (i) participants on average contribute between 40% and 60% of their endowment in one-shot encouters and that (ii) average contributions start out at 40% – 60% of the endowment but decrease over time in finitely repeated interaction. Neither observation is consistent with the assumption that all participants are rational and seek to maximize their own monetary outcomes and that this is commonly known.[14]

[14]For the first observation, this is obvious since full defection is the dominant solution in the one-shot game. For the second observation, the statement follows from a classical backward

A rather recent advancement is the understanding that a large fraction of participants in public goods games are *conditional cooperators* who prefer to cooperate if they know that other actors cooperate as well. The most clear-cut evidence on conditional cooperation stems from experiments that elicited subjects' willingness to contribute conditional on the average contribution in their group. In a one-shot linear public goods game, Fischbacher et al. (2001) find that 50% of all participants are conditional cooperators, whereas only 30% are *free riders* who contribute nothing irrespective of the decisions of other actors.[15] Fischbacher and Gächter (2010) replicate this result.[16] For an extensive survey of experimental and field evidence on conditional cooperation, see Gächter (2007). Conditionally cooperative behavior is consistent with various theoretical approaches which use psychological game theory (Geanakoplos et al., 1989) to model intention based reciprocity. This has been shown by Rabin (1993) and Dufwenberg and Kirchsteiger (2004) for the simultaneous and sequential prisoner's dilemma game, respectively. Theoretical models that incorporate a dislike for inequitable distributions of monetary outcomes such as in Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) are also able to explain conditionally cooperative behavior (see Fehr and Schmidt, 2006, p. 669).[17] For the large part of this thesis, we refrain from discussions which preferences underlie conditional cooperation. Instead, the conditionally cooperative attitude of some part of the population is taken as a given and serves as an assumption when solutions for social dilemmas are discussed.

The presence of conditional cooperators has fundamental consequences for behavior in social dilemmas. For groups that consist of conditional cooperators the game above – although a linear public goods game in monetary payoffs – may even be a coordination game in utility payoffs. To see this, let us consider a subject that has a preference to match the average contribution of all group members

induction argument: since every last period is strategically equivalent to one-shot interaction, zero contributions are the dominant choice. Rational decision makers should anticipate such behavior. Consequently, the second to last period is, again, strategically equivalent to one-shot interaction. By this argument, full defection unravels "from behind" until the first period.

[15] The third notable group displayes *hump-shaped* patterns, in that they conditionally cooperate for low average group contributions and reverse this behavior for large average contributions.

[16] For experiments that compare the proportions of free riders and conditional cooperators in different cultural areas, see Kocher et al. (2008) and Herrmann and Thöni (2009).

[17] According to Fischbacher et al. (2001, p. 397), "[c]onditional cooperation can be considered as a motivation in its own [...]." For an argument that views conditional cooperation as a social norm, see Fehr and Fischbacher (2004).

excluding herself.[18] This subject prefers, e.g., to contribute her entire endowment if the other group members do the same. In a homogenous group of such subjects every uniform contribution vector constitutes an equilibrium of the game defined by the subjects' utilities. Thus, a linear public goods game with full free riding as the only equilibrium in monetary terms becomes a coordination game with Pareto-ranked equilibria in utility terms. The idea that the actual preferences of the subjects may transform cooperation games into coordination games is by no means a novelty of this thesis. It has been voiced, inter alia, by social psychologists (see, e.g., Dawes et al., 1988), sociologists (see, e.g., Simpson, 2006), economists (see, e.g., Sen, 1967, Fehr and Schmidt, 2006, Gächter, 2007), and legal scholars (see, e.g., Kahan, 2005). It is also consistent with interdependence theory in social psychology (Kelley and Thibaut, 1978, Kelley, 1979) which argues that the (experimentally) "given" payoff matrix may diverge from the "effective" matrix and that the latter is directly linked to behavior. Kollock (1998b) uses this concept to show that a prisoner's dilemma game in monetary payoffs can be an assurance (or stag hunt) game in utility payoffs. The same transformation is shown in Rabin (1993) who uses the concept of "fairness equilibrium."

In repeated interaction, the presence of conditional cooperators may even create incentives for rational and self-regarding decision makers to act cooperatively. This is possible since conditional cooperative behavior increases the long-term expected payoff for cooperative choices by other actors. Given appropriate beliefs with regard to the proportion of conditional cooperators in the group, a rational and self-regarding actor may prefer to start by cooperating, in order to build up a good reputation, and free ride later on (Kreps et al., 1982). Another implication of the presence of conditional cooperators is that belief management may take on an important role for how to approach the free-rider problem (Gächter, 2007). In order to boost cooperation, leaders may, among others, attempt to manipulate the beliefs which followers hold with respect to other actors' contribution decisions. The concept of leadership is the topic of the following section.

[18] In Fischbacher et al. (2001), such subjects are called "perfect conditionally cooperative."

1.3 A Leadership Perspective

Concepts of leadership have been discussed in many social sciences (for overviews, see House and Baetz, 1979, Yukl, 2002). In economics, effective leadership is especially important in organizational contexts where it serves as a means to stimulate efforts from staff when employment contracts are incomplete (for example for intellectual work).

The literature offers a plethora of definitions for leadership. House and Baetz (1979), e.g., count 70 definitions, Yukl (2002) reports on 13. We subscribe to the notion proposed by Max Weber (see Gerth and Mills, 1958, p. 148 – 150) who argues that the essence of leadership is inducing others to follow voluntarily. In our view, leadership is thus distinct from formal authority in that it does not rely on coercive means to influence behavior.[19] We assume that leaders have neither sticks nor carrots at their disposal. In fact, we do not even require a hierarchy to be in place for somebody to exert an influence on others. In essence, everybody can be a leader. This notion of leadership has thus some resemblance to the literature on peer effects (see, e.g., Falk and Ichino, 2006).[20]

In collective action problems, leadership can be a means to motivate cooperative behavior (see, e.g., Potters et al., 2005). Experimental economists investigate leadership predominantly in the context of linear public goods games in which actors decide upon their contributions in the following order. The leader chooses her contribution first. Her decision is communicated to multiple followers who then choose their contributions simultaneously (see, e.g., Gächter and Renner,

[19] In fact, the power to dictate other actors' behaviors is rarely absolute. Even in a military context, one might argue that soldiers have some discretion on how to fulfill their duty. Thus, the notion of leadership we discuss might also extend to situations in which some degree of formal authority seems feasible.

[20] Research on leadership also figures prominently in social psychology. A distinguishing feature of this literature is the role it assigns to the self-concepts of leaders and followers – aspects that this thesis neglects (for a survey, see, van Knippenberg et al., 2004). De Cremer and van Knippenberg (2002), e.g., show that charisma or fairness of treatment of leaders may affect a follower's self-concept by eliciting group identification. Another notable observation is that prototypicality, i.e., a close correspondence of a leader's and a group's characteristics, is conducive to effective leadership (see, e.g., Hogg, 2001). A marked difference between studies on leadership in psychology and those presented in this thesis is that while, in the former, leaders are mostly presented by verbal descriptions (see, e.g., De Cremer and Van Vugt, 2002, De Cremer and van Knippenberg, 2002), in the latter, followers are asked to respond to actual behavior.

2004, Güth et al., 2007, Levati et al., 2007). This conceptualization is clearly in accordance with the notion of leadership given above since its effectiveness in alleviating the free-rider problem relies on leaders' willingness to give good examples and followers' voluntary responses. Leading by example can also be considered a motivational solution to collective action problems since, in a group of rational and self-regarding actors, followers would not follow and, consequently, leaders would not lead. In Chapter 5, we generalize leading by example to a scenario of intergroup competition. Leading by example is, however, only one possible form of leadership that relies on other-regarding motivations. In a series of studies which are presented in Chapters 2 to 4, we investigate "leading by words." The context is a linear public goods game with sparse cheap talk communication, where only one member of a group is able to send a written message to the other group members. The written messages allow for persuasion in favor of the collective interest and, as our results show, can be used to foster cooperation. As with leading by example, the effectiveness of leading by words relies on followers' voluntary responses to one group member's attempt to stimulate cooperation. The obvious difference is that the act which may motivate cooperation is a committed costly action in one case and merely cheap talk in the other.

Economic theory largely neglects the notion of leadership. In one of the few models, Rotemberg and Saloner (1993) examine the influence of leadership style (participatory versus autocratic) on firm profitability assuming that leaders may differ in empathy. Hermalin (1998) investigates leading by example or sacrifice using the teams model formulated by Holmstrom (1982). He assumes uncertainty over the value of the common project and asymmetric information in that the true value is only known to leaders.[21] A leader's effort is thus informative for followers as it may signal the value of the common project.[22] In two follow-up studies, Hermalin (2007) and Komai et al. (2007) extend Hermalin's model to a scenario of repeated interaction and provide a rationale for why leaders should not simply disclose their superior information. Huck and Rey-Biel (2006) consider sequential contribution decisions in collective action problems with two actors and complete information about the game. Their model provides an explanation why

[21] Andreoni (2006) develops a model of leadership giving in fund raising that also builds on information asymmetries – in this case about the quality of charities.

[22] Potters et al. (2007) provide an experimental investigation of Hermalin's model.

good examples may foster cooperation based on preferences for conformity in contributions, or, in other words, preferences for conditional cooperation. In a nutshell the model shows that reciprocal behavior of a conformist second mover generates an incentive for first movers to give a good example.

The model by Huck and Rey-Biel (2006) cannot be directly applied to most experimental studies on leading by example since it only considers two actors, one leader and one follower. In this setting, followers no longer face any uncertainty after being informed about the leader's contribution. In cases with more than one second mover, Huck and Rey-Biel's line of thought has to be extended to incorporate followers' beliefs about other followers' actions. Given the presence of conditional cooperators, this is viable: a first mover's contribution may affect the expectations of second movers. Conditionally cooperative followers should be sensitive to changes in beliefs, which is what may create an incentive for a first mover to give a good example.[23] This notion of leaders as "belief managers" (Gächter, 2007, p. 36) is also applicable to leading by words since one-way communication can be used to make a specific contribution decision a salient choice. If followers are conditional cooperators and believe that some other conditional cooperators are present, one-way messages may affect their expectations and, consequently, their contribution decisions.

The following section sketches the motivation, the experimental design, and the results of each of the studies presented in this thesis.

1.4 Overview

Chapter 2 is entitled "Leading by Words: A Voluntary Contribution Experiment with One-Way Communication." It is joint work with Anastasios Koukoumelis from the Max Planck Institute of Economics, Jena, Germany and Vittoria Levati from the Max Planck Institute of Economics, Jena, Germany and Verona University, Italy. In this chapter, we seek to advance the understanding why costless, non-binding, and non-verifiable communication enhances voluntary cooperation in experimental public goods games. At present, the leading explanation for this effect is that communication allows for a mutual exchange of promises to cooper-

[23] For a similar argument on coordination games, see Foss (2001).

ate and that these pledges are reinforced by the social norm of promise-keeping (Bicchieri and Lev-On, 2007). This explanation is consistent with the view that communication allows conditional cooperators to coordinate on specific contribution decisions (Fehr and Fischbacher, 2002, Mohlin and Johannesson, 2008). Mutual promises are, however, only one possible coordination device. This raises the question whether mutual promises are necessary for communication to enhance voluntary cooperation. Attempting to answer this question, we designed an experiment which allows for communication but negates the opportunity to exchange mutual promises. More specifically, we use a one-way communication protocol that enables only one member of a group to send a written message to her fellow players. After the message is received, all group members decide privately and independently on their contributions to the public good. Our results show that one-way communication enhances cooperation with respect to no-communication baseline treatments in repeated as well as one-shot interaction. Moreover, in repeated interaction, we do not observe differences in behavior between treatments that either allow for one-way communication before each and every period or only before the first period. An analysis of the communication content reveals that most senders of messages give specific contribution suggestions and that the majority of participants follows these suggestions. Our results are thus consistent with the notion that communication allows for coordination in public goods games. Moreover, they clearly demonstrate that a mutual exchange of promises is not necessary for communication to enhance voluntary cooperation.

Chapter 3 is entitled "A Voluntary Contribution Experiment With One-Way Communication and Endowment Asymmetry." It is, again, joint work with Anastasios Koukoumelis and Vittoria Levati. Herein, we extend the study presented in Chapter 2 in order to allow for asymmetry in endowments. In laboratory experiments, symmetric games are the standard case and often a good starting point. Disregarding asymmetric decision situations may however result in an incomplete, probably even inaccurate understanding of human behavior (Güth et al., 2004). In fact, outside the laboratory, asymmetry seems to be the rule rather than the exception.[24] In collective action problems, you can think of asymmetry for

[24]One may even argue that fully symmetric games are models of a class of decision situations

example in the valuation of the common project, the information structure, or even dimensions which are completely unrelated to the decision situation (Anderson et al., 2008). We consider asymmetry in endowments as a proxy for "deep or small pockets" regarding problems of public good provision or differences in time constraints for work on joint projects. This particular form of asymmetry has been shown to reduce voluntary cooperation in experiments with and without (multi-way) communication (Isaac and Walker, 1988a, Cherry et al., 2005) and may impair the effectiveness of one-way communication since, in such a setup, the efficient provision of the public good requires unequal contributions. Our results show that one-way communication is an effective mechanism to increase and sustain voluntary cooperation even when endowments are asymmetric. A particularly notable observation is that, while behavior is broadly consistent with an equal contribution rule when communication opportunities are absent, it is broadly consistent with an equal payoffs rule when one-way communication is allowed.

Chapter 4 is a single-authored project entitled "Leading by Words in Privileged Groups." This study reports on a series of one-shot experiments which are designed to investigate whether one-way communication is effective if messages cannot relate to the experimental game or if cooperation generates payoff inequalities. The first question is intended to clarify whether one-way communication affects behavior simply by generating a more social decision environment. To resolve this question, we compare contribution behavior in three treatments: a no-communication baseline and two communication conditions, only one of which allows for messages that are related to the experimental game. Since our results show that only game-relevant one-way communication increases cooperation, we conclude that the mechanisms which underlie the effectiveness of one-way messages have to reside within the communication content. The most prominent observation concerning the communication content in Chapters 2 and 3 is that messages incorporate suggestions to contribute the entire endowment. A common feature in these experiments is that efficient contributions by all group members also yield equal monetary payoffs. This property might be critical for the effectiveness of one-way communication, specifically if subjects dislike un-

which have no examples in everyday human interactions.

equal distributions of payoffs. In order to assess whether (game-relevant) one-way communication may also enhance cooperation if efficient contributions lead to unequal earnings, we compare the effectiveness of one-way messages in normal, i.e., symmetric, vis-à-vis privileged groups (Olson, 1965). The latter differ from the former only in that exactly one (high-benefit) group member gets an additional remuneration for each unit of the public good that is provided. Consequently, uniform contributions are associated with equal earnings in normal and unequal earnings in privileged groups. Our results show that the presence of the high-benefit member inhibits contributions by all other actors when communication opportunities are unavailable. One-way communication, however, enhances overall cooperation as it boosts contributions particularly by those actors who do not benefit disproportionally from the public good. This behavioral change occurs despite the fact that messages originate from the high-benefit members and are therefore suspected to serve egoistic purposes.

Chapter 5 is a single-authored project entitled "Leading by Example in Intergroup Competition: An Experimental Approach." In this study, we envisage to augment the research on leading by example in two respects. First, we make use of the strategy method when eliciting followers' contribution decisions. This allows us to categorize followers into types based on their conditional responses to a leader's contributions. Second, we generalize leading by example to a scenario of intergroup competition. Bornstein et al. (1990), Bornstein and Ben-Yossef (1994), and Halevy et al. (2008) show that intergroup competition has two distinct (positive) effects on intragroup cooperation – one that works via monetary incentives and one that operates as it affects the willingness to act in the interest of the own group. Our implementation of intergroup competition controls for the former effect but allows for the latter. The experiment consists of two stages. In the first stage, interaction is one-shot and followers' choices are elicited via the strategy method. In the second stage, groups interact for ten periods using the play method. The results from the first stage suggest that the usually observed undercutting of the leaders' examples may be the result of type-specific behavior. While many participants are conditional followers who behave in a reciprocal way to a leader's contributions, we also observe strict non-contributors, unconditional contributors, and hump-shaped followers. A small fraction of followers

even punishes better examples by means of reverse conditional behavior. The presence of intergroup competition has differentiated effects. Leaders' decisions remain unaffected. Followers, by contrast, react more cooperatively to a leader's contributions. The results from the second stage indicate that leading by example is an effective means to foster intragroup cooperation in a scenario of intergroup competition. In fact, at least in our data, leading by example does not enhance cooperation in isolated groups but only in conjunction with intergroup competition.

Chapter 6 briefly summarizes the results of this thesis and provides concluding remarks.

CHAPTER 2

LEADING BY WORDS: A VOLUNTARY CONTRIBUTION EXPERIMENT WITH ONE-WAY COMMUNICATION*

*This chapter is based on the article "Leading by words: A voluntary contribution experiment with one-way communication" by Koukoumelis, Levati, and Weisser, 2012, Journal of Economic Behavior & Organization 81 (2), 379–390. The working paper version can be found in Koukoumelis et al., 2009, Jena Economic Research Papers 106. The authors thank the co-editor of JEBO, Catherine Eckel, and three anonymous referees for helpful comments and suggestions. We also thank seminar participants at the following universities and conferences: Bolzano, Munich, Siena, Verona, ESA 2010 in Copenhagen, and IAREP/SABE/ICABEEP 2010 in Cologne. We benefited from discussions with Anna Conte, Christoph Engel, Simon Gächter, Werner Güth, David Hugh-Jones, Oliver Kirchkamp, Martin Strobel, and Marie Claire Villeval. Matthias Uhl provided valuable assistance in the analysis of the messages' content.

2.1 Introduction

One of the most consistent experimental findings in the social dilemma literature is that costless, non-binding, and non-verifiable communication (i.e., cheap talk) has a positive effect on cooperation.[1] But what is it about communication that boosts cooperation? Three aspects of communication have been suggested in the literature as inductive to cooperation (see, e.g., Dawes et al., 1977): identification, discussion, and commitment. Several experimental studies demonstrate that neither mere identification nor discussion is *sine qua non* for the communication effect to take place (see, e.g., Bouas and Komorita, 1996, Bohnet and Frey, 1999, Brosig et al., 2003). Instead, the commitment to cooperate, in the form of a *mutual* exchange of promises and pledges, is considered crucial for the cooperation-enhancing effect of communication (see Kopelman et al., 2002 and Bicchieri and Lev-On, 2007 for surveys of relevant work in the psychology and economics literature, respectively).[2]

Most of the evidence on the role of commitment comes from two kinds of studies. First, experiments that draw a comparison between face-to-face and other forms of active communication (e-mails, chat-rooms, audio-conferences, numerical cheap talk; see, for instance, Frohlich and Oppenheimer, 1971, 1998, Brosig et al., 2003, Bochet et al., 2006). Such experiments find that the strength of the communication effect depends on the communication medium, with a stronger effect of face-to-face discussion compared to any other alternative. The crucial factor here is that face-to-face discussion facilitates the exchange of mutual promises.[3] Notice, however, that all these communication methods do allow subjects to exchange non-binding promises.[4] Second, experiments that draw a comparison between face-to-face and passive communication (see, e.g., Brosig

[1] Sally (1995) offers a meta-analysis of 35 years of social dilemma experiments and shows that enabling people to communicate increases cooperation rates significantly. Balliet (2010), in a more recent meta-analysis, reports similar results.

[2] A notable exception is Bochet and Putterman (2009).

[3] According to Bicchieri and Lev-On (2007, p. 145), "using computer-mediated communication instead of face-to-face communication can hamper the generation of normative settings in which promises are perceived as reliable."

[4] Although numerical cheap talk (Bochet et al., 2006) may be regarded as an exception, it is impossible to exclude that the announced contribution level is perceived as a signal for how much one intends to contribute, and hence as a promise.

et al., 2003).[5] This approach prevents commitment at the cost of rendering the source of the messages external to the group.

An unambiguous way of studying whether commitment is necessary for cooperation, in the sense that the effect of communication vanishes in its absence, calls for a setting where mutual pledges to cooperate are ruled out by design while the in-group communication channels remain intact. In this study we provide a series of experimental studies based on such a setting.

We consider a linear public goods game with one-way communication. All group members make their contribution decisions privately and simultaneously. But prior to this, one of them, a group member that is randomly assigned the role of "communicator," can send a free-form text message to his fellow players. In this sense, the communicator is a leader who leads by words.[6] More importantly, such a unidirectional messaging precludes the mutual exchange of promises. Hence, if the latter were necessary for the rise in cooperation rates, we would not observe any difference in contribution levels in comparison to a no-communication baseline treatment. If, on the other hand, giving someone the chance to suggest and rationalize cooperative play suffices to alleviate the free-rider problem, then the communicator's presence should promote contribution towards the public good.

We would like to stress that we are not questioning the importance of verbal commitment.[7] We are testing instead whether commitment is strictly necessary for the cooperation-enhancing effect of communication. One other plausible explanation for this effect is *coordination* (see, e.g., Charness, 2000, Mohlin and Johannesson, 2008). Public goods experiments are usually parameterized so that

[5] Passive communication means that subjects may attend but not intervene in the communication between outsiders (that is, people that do not belong to their own group). The exogenous contribution assignments studied by, e.g., Seely et al. (2005) are akin to passive communication.

[6] With a few exceptions, experimental economists investigating the effects of leadership in social dilemma games focused on leading by example (see, e.g., Güth et al., 2007, Levati et al., 2007). Pogrebna et al. (2009) consider a public goods game where first the leader can promise to contribute a certain amount and then all group members make binding contribution decisions. The authors compare leading by pre-game communication with leading by example and find no dependence of contributions on the leadership style.

[7] Gneezy (2005), Charness and Dufwenberg (2006), and Vanberg (2008), among others, show that people have a preference for keeping their word. Ellingsen and Johannesson (2004) provide experimental evidence that promises can mitigate the hold-up problem.

the dominant strategy of a homogeneous population of monetary payoff maximizers is to free ride. The systematic finding of positive contributions has therefore led researchers to assume that people exhibit social preferences. Preferences for conditional cooperation (in the sense that people prefer to cooperate as long as all others cooperate), in particular, can transform social dilemmas into coordination games.[8] Whenever there are multiple equilibria, as it is the case with coordination games, the communicator may affect the others' play by drawing their attention to a specific equilibrium (see, e.g., Cooper et al., 1992, Farrell and Rabin, 1996, Crawford, 1998, Brandts and Cooper, 2007).

We investigate how effective the communicator is in fostering contributions by means of a series of finitely repeated games where the number of communication opportunities acts as the primary treatment variable. The baseline treatment involves no communication at all. The participants in the other treatments can communicate either prior to each and every period (continuous communication) or exclusively prior to the first period (pre-play communication). We compare the results of these treatments to assess not only the effectiveness of one-way communication but also its dependence on the frequency of messaging.

In a repeated context, incomplete information about the other players' types allows for reputation building. Given appropriate beliefs, a rational course of action for a self-regarding forward-looking player would be to start by cooperating, in order to build a good reputation, and free ride later on (Kreps et al., 1982). Such strategic reasoning might interact with the communicator's potential to affect behavior. To assess the importance of reputation in the workings of one-way communication, we run a series of one-shot games where people have no strategic incentives to contribute.

Whether and to what extent one-way communication, in the form of leading by words, affects cooperation levels in social dilemmas could be of interest to group organizers and institution designers. As noted for example by Messick and Brewer (1983), multi-way communication in real-world social dilemmas can be very costly, or even unfeasible.[9] Yet, if – as our results indicate – one-way

[8] This has been shown by, among others, Sen (1967), Levati and Neugebauer (2004), and Levati (2006).

[9] Most social dilemmas are large group problems (e.g., global environmental problems) affording participants little or no opportunity at all to either communicate or negotiate a solution.

communication increases cooperation and pre-play communication is as effective as communication on a repeated basis, then the required organizational cost may be lower than presumed.

The chapter is organized as follows. Section 2.2 lays out our experimental design and details our research questions. Sections 2.3 and 2.4 provide analytical results on the finitely repeated and one-shot treatments, respectively. Section 2.5 concludes.

2.2 The Experiment

2.2.1 The Basic Public Goods Game

The basic game is the voluntary contribution mechanism (see, e.g., Isaac et al., 1984). Let $I = \{1, \ldots, 4\}$ stand for a group of four participants who interact for $t = 1, \ldots, T$ periods in a partners design (that is group composition does not change throughout the experiment). At the beginning of every period, each individual $i \in I$ is endowed with 25 ECU (Experimental Currency Units) which he can either consume privately or contribute to a public good. Denoting i's contribution level by $c_{i,t}$, where $0 \leq c_{i,t} \leq 25$, his monetary payoff per period is given by:

$$\pi_{i,t}(\mathbf{c}_t) = (25 - c_{i,t}) + 0.4 \sum_{j=1}^{4} c_{j,t} \;\; \forall \; i, t, \tag{2.1}$$

where $\mathbf{c}_t = (c_{1,t}, \ldots, c_{4,t})$ and $0.4 \sum_{j=1}^{4} c_{j,t}$ represent the period t strategy profile and income from the project, respectively.

Since the marginal per capita return is less than unity, the dominant strategy for a self-regarding person (i.e., a monetary payoff maximizer) in a one-shot interaction is to contribute nothing. If all group members free rided, then each one of them would earn 25 ECU. On the other hand, the socially efficient outcome (i.e., the outcome that is maximizing the sum of $\pi_{i,t}(\mathbf{c}_t)$ over $i = 1, \ldots, 4$) is to contribute everything. If all group members made the socially efficient choice, then each one of them would earn 40 ECU. Assuming common knowledge of rationality and preferences to maximize monetary outcomes, the dominance of free riding extends to the finitely repeated game.

2.2.2 Treatments and Research Questions

Using a between-subjects design, we study five treatments that build on the basic game described above. The treatments differ with respect to the number of repetitions and, in the case of repeated games, the frequency of communication.

Finitely Repeated Games

In the first three treatments, participants interact for ten periods. The characteristics of these treatments are as follows:

Baseline (B_{10}): Group members cannot communicate with each other. In each period, they decide simultaneously and privately on the number of ECU that they want to contribute to the public good.

Continuous Communication (CC): At the beginning of the experiment, one member of each group is randomly appointed communicator (a role which he retains throughout the experiment). Prior to each period, the communicator is given the opportunity to send a written message to his co-players (details are given below). To ensure that the message is non-verifiable (and thus cheap talk), the communicator's contribution level is not identifiable.

Pre-play Communication (PC): The (randomly selected) communicator can send just one written, non-verifiable message prior to the first period (i.e., in advance of any decision making). Afterward, group interaction follows B_{10}.

These treatments are expressly designed to address the following questions:

Question 1. *Does one-way communication affect contributions towards the public good?*

Question 2. *Is the number of communication periods relevant, i.e., does the effect of one-way communication depend on whether written messages are sent repeatedly or just once?*

Question 3. *What kind of arguments are invoked by the communicator and how do they influence behavior?*

Table 2.1: Research Questions and Appropriate Answering Approaches

Research question	Answering approach
(1) Is one-way communication effective?	Compare CC with B_{10} Compare PC with B_{10}
(2) Does communication frequency matter?	Compare CC with PC
(3) What kind of arguments are used?	Study the messages' content
(4) Does strategic play matter?	Compare C with B_1

Note: Questions 1 to 3 are addressed in the context of finitely repeated games, Question 4 in the context of one-shot games.

The correspondence between these research questions and the methods used to address them is displayed in the upper panel of Table 2.1.

With regard to Question 1, a number of papers have addressed the theoretical conditions under which augmenting the game with cheap talk assists in the achievement of efficient outcomes (see Farrell and Rabin, 1996, Crawford, 1998 for surveys). Whenever individual and group interests conflict completely (as in our case), cheap talk is not expected to alter the prediction of full free riding insofar as people care only about their own monetary payoff. However, this prediction has been contradicted by decades of experimental research, with commitment being regarded as the most likely explanation for the effect of communication.

We argue that there is more to the communication's impact on cooperation than the behavioral importance of promises to cooperate. More precisely, the communicator may enhance cooperation by providing a mechanism that allows for coordination on the socially efficient outcome. Previous experimental work indicates that people are often motivated to contribute to the public good provided that others do the same (see, e.g., Keser and van Winden, 2000, Fischbacher et al., 2001, Levati and Neugebauer, 2004, Croson, 2007, Gächter, 2007, Fischbacher and Gächter, 2010). This kind of conditionally cooperative preferences can transform the social dilemma game into a coordination game with multiple Pareto-ranked equilibria.[10] That the communicator's cheap talk increases the amount of

[10]Sen (1967) shows that if players are conditional cooperators a prisoner's dilemma in material payoffs can turn into an assurance game in subjective payoffs. Distributional preferences à la

efficient play in coordination games is consistent with theory and experimental evidence (Farrell and Rabin, 1996, Cooper et al., 1992, Crawford, 1998, Charness, 2000, Blume and Ortmann, 2007). A message urging cooperation may positively influence contributions also because it reduces the uncertainty about the numbers of conditional cooperators in the group.[11] On the basis of these arguments we conjecture a positive answer to Question 1.

As to Question 2, the existing research contrasting pre-play with continuous communication in social dilemma experiments, albeit founded on multidirectional communication, yields mixed results. While certain studies conclude that pre-play communication has a lasting effect on cooperation (Brosig et al., 2003, Balliet, 2010), others find that cooperation rates decline in response to limitations in the opportunities to communicate (Ostrom et al., 1992, Frohlich and Oppenheimer, 1998).

With one-way communication, two conjectures point to a negative answer to Question 2: (i) the communicator is able to coordinate actions on the efficient outcome at the outset of the game; and (ii) high first-period contributions are critical for future contribution levels. The first conjecture is plausible provided that, as mentioned earlier, preferences for conditional cooperation can transform the social dilemma into a coordination game. Blume and Ortmann (2007), among others, claim that in games with multiple Pareto-ranked equilibria, communication reduces the first-period variance of play by concentrating action near the unique efficient equilibrium. Empirical support for the second conjecture is provided by, e.g., Keser and van Winden (2000), who, having compared partners with strangers conditions, document the importance of first-period play on the whole game. Based on (i) and (ii), we expect the communicator to serve as an enduring coordination device, in other words to have a lasting positive effect on contributions.[12]

Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) predict similar results.

[11] We thank an anonymous referee for bringing this to our attention.

[12] It might be claimed that additional communication opportunities are helpful, having restart effects, if contributions start decreasing. Yet, anticipation of these effects may tempt individuals into earlier free riding: since the communicator's message can help revive cooperation, reducing one's own contributions in a period does not necessarily lead to a reaction from the others in the next period. Such a behavioral pattern is observed, for instance, by Bochet et al. (2006).

Finally, we have no preconceptions about Question 3.[13] However, we would expect from a communicator interested in coordinating his co-players' actions to make an effort to persuade them of the advantages of the socially efficient contribution.

One-Shot Games

Here, participants interact just once.

Baseline (B_1): The group members cannot communicate with each other. They make a one-shot contribution decision.

Communication (C): Before the one-shot interaction, one member of each group is randomly appointed communicator and can send a message to his co-players.

These two treatments are designed to address the following question (see as well the lower panel of Table 2.1):

Question 4. *What is the effect of one-way communication when subjects are denied the opportunity to play strategically?*

In finitely repeated games, if the information about types is incomplete, strategic reputation building may by itself bring about more cooperation. For example, suppose that a self-regarding player believes that some other players in his group are conditional cooperators and that the communicator's message could coordinate them on a specific contribution. Then, it may be optimal for him to make that same contribution early in the game (so as to induce these conditional cooperators to contribute) but free ride later on (Kreps et al., 1982, Andreoni, 1988, Sonnemans et al., 1999). With one-shot interaction there are no incentives for such a forward-looking behavior. However, the coordination role of the communicator is active in treatment C. Hence, by comparing contributions in B_1 and C, we can assess whether one-way communication is effective in the absence of strategic reasoning.

[13] Previous studies analyzing the content of communication either involve multi-directional communication or consider games where promises to cooperate play a crucial role (see, e.g., Charness and Dufwenberg, 2006, Brandts and Cooper, 2007, Sutter and Strassmair, 2009).

2.2.3 Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted in the experimental laboratory of the Max Planck Institute of Economics (Jena, Germany). The subjects were undergraduate students from the Friedrich Schiller University Jena. They were recruited using the ORSEE (Greiner, 2004) software. Upon entering the laboratory, the subjects were randomly assigned to visually isolated computer terminals. The instructions were distributed and then read aloud to establish common knowledge.[14] All subjects' questions were answered individually at their seats. Before starting the experiment, subjects had to answer a control questionnaire which tested their comprehension of the rules.

Whenever communication was allowed, the communicator could use a text box to type in his message.[15] He had a maximum of four minutes to compose the message, but it was at his discretion to send it ahead of the deadline. In principle, the form of the message was free, the only restrictions to its content being that the communicator could neither identify himself, nor promise side-payments, nor threaten the other group members with anything that could occur after the experiment. To enforce compliance with these restrictions, all messages were screened before being sent.[16] Then, all of them were delivered simultaneously. It was common knowledge that (a) the messages were cheap talk (i.e., costless and non-binding), (b) all group members received exactly the same message from the group communicator, and (c) only after having read the communicator's message could the group members decide simultaneously on their individual contributions.

[14] All instructions can be found in Appendix A.2.

[15] We prefer free-form text messages to face-to-face communication so as to isolate the impact of the message's content from visual (i.e., body language, eye contact, facial expressions) and verbal cues (tone of voice, phrasing, fluency, manner of expressing moral rhetoric). We prefer free-form to pre-specified messages so as to allow subjects to express freely their thoughts and views of the game. Lundquist et al. (2009) notice that "using pre-fabricated messages in experiments will not capture the full effect of communication." Similarly, Bochet and Putterman (2009) and Charness and Dufwenberg (2010) do not find any communication effect using pre-fabricated messages incorporating promises.

[16] Improper messages were to be screened out and their sender was supposed to be given a warning for misconduct, but as a matter of fact such a thing never happened.

Finitely Repeated Treatments

At the end of each period, participants got feedback on (a) the number of ECU contributed by each group member (with the individual contributions being sorted in descending order), (b) the income from the project, and (c) their corresponding payoff. Payoffs were quoted in ECU, where 10 ECU = 50 euro cents. At the end of the last period participants were paid in private their accumulated earnings. Average earnings per subject were 20.60 euro (inclusive of a 2.50 euro show-up fee), ranging from 13.8 euro in treatment B_{10} to 23.6 euro in treatments CC and PC.

One-Shot Treatments

One-shot treatments started with six training periods that involved neither interaction (the others' decisions were selected randomly by the computer) nor communication. The sole aim of these periods was to familiarize the participants with the game and its incentives (no payments were associated with them).

Participants in these treatments received the same information as in the repeated treatments at the end of the experiment. The exchange rate was 10 ECU = 400 euro cents. Participants earned on average 15.12 euro, inclusive of a 2.50 euro show-up fee.

2.3 Results on the Finitely Repeated Treatments

We ran three sessions per treatment (B_{10}, CC, and PC). Each session involved 24 participants. With group size equal to 4, we have 18 independent observations per treatment. The results are presented in two parts: first, the effects of one-way communication on contribution levels; and second, the communication's content and its relation to contribution decisions.

Table 2.2: Summary Statistics of Average Group Contributions Across our Finitely Repeated Treatments

Treatment	Mean	Std. dev.	Median	MAD
B_{10}	12.27	8.73	11.62	11.68
CC	22.15	5.94	25.00	0.00
PC	21.75	6.68	25.00	0.00

Note: 180 observations per treatment.

2.3.1 The Effects of One-Way Communication

Table 2.2 presents descriptive statistics of average group contributions.[17] The mean and median of the series in the CC and PC treatments are notably larger than their respective values in the B_{10} treatment. In addition, the standard deviation is smaller, which should not be surprising given that for CC and PC the median average group contribution is equal to the maximum contribution, namely 25.[18] The disparity in dispersion between the baseline and the treatments allowing communication becomes more pronounced once we consider the median absolute deviation about the median (MAD) as a robust measures of scale.[19]

Figure 2.1 shows how the time series of measures of location of the average group contributions respond to changes in our treatment variable. In panel A, the baseline treatment replicates standard findings (see, e.g., Ledyard, 1995): the mean of the average group contributions begins at 57.1% of the endowment and declines with repetition (in the last period it stands at 18.8% of the endowment). In contrast, in the treatments allowing communication the mean starts at very high levels (89.6% and 90.0% of the endowment in CC and PC, respectively) and remains fairly stable in all periods but the last (its period 9 value is 85.4% of the endowment in CC and 83.3% of the endowment in PC). This stability is clearer if we acknowledge that in the treatments allowing communication the distributions

[17] The CC and PC independent observation series contain outlying observations at the lower tail of their distributions. Thus, in what follows, besides the conventional descriptive statistics (mean and standard deviation) we also report measures of location and scale which are robust to the presence of outliers.

[18] In fact, 73.33% (72.78%) of the average group contributions in the CC (PC) treatment equal the subjects' endowment. The corresponding percentage in the B_{10} treatment is 20.56%.

[19] If $\{x_1, \ldots, x_n\}$ is a set of numbers, $\text{MAD} = b\ \text{med}_i|x_i - \text{med}_j x_j|$ (where med stands for median and b is a correction factor for consistency) is the most frequently used robust estimate of scale.

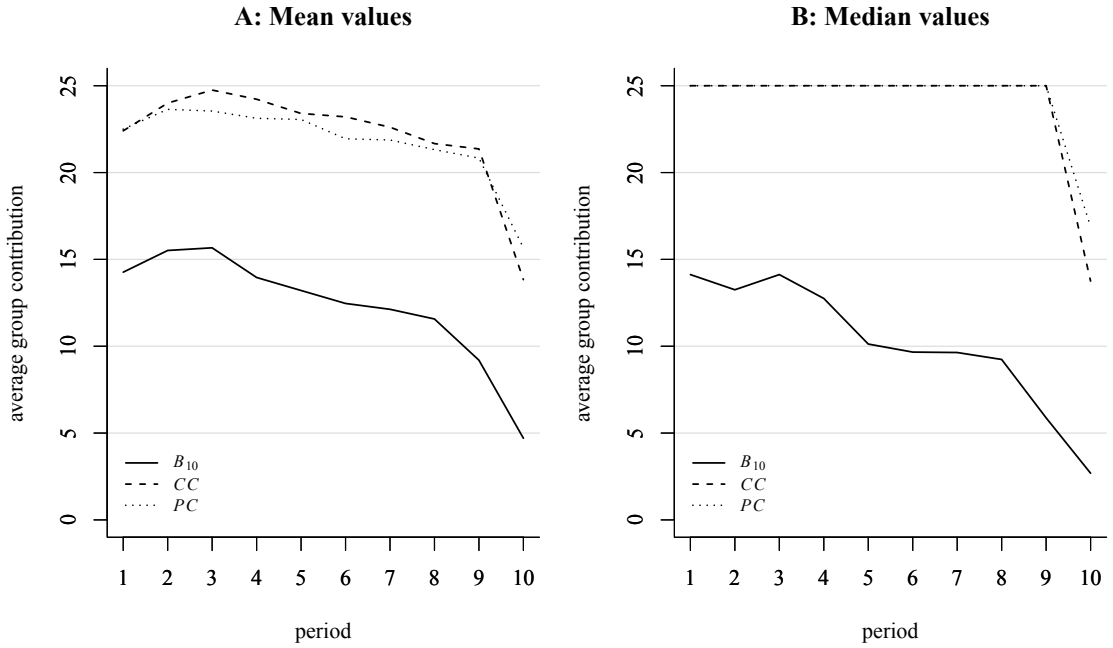


Figure 2.1: Mean and Median of Average Group Contributions Over Time (Finitely Repeated Treatments; 18 Observations per Period)

of the average group contributions in each period are skewed to the left (i.e., they have relatively few low values, see Figure 2.2), and opt for their median values as better indicators of their central tendency. In periods 1 to 9, the medians of the average group contributions in the CC and PC treatments equal 25 (see Figure 2.1 B). Furthermore, it is in period 9 that the difference between the median series of the baseline and the treatments allowing communication reaches its maximum value (that is 19.1 ECUs, or, alternatively, 76.5% of the endowment).

One-sided Wilcoxon rank sum tests with group contributions averaged over all 10 periods as independent observation units confirm that the communicator's presence raises contribution levels significantly ($p < 0.01$ in both CC vs. B_{10} and PC vs. B_{10} comparisons). The same holds if we compare average group contributions in any particular period; all p-values are well below the conventional significance levels (the largest of them, equal to 0.003, is associated with the sixth period comparison between B_{10} and PC). On the other hand, the frequency of communication opportunities does not appear to have any significant effect: it is not possible to reject the null hypothesis that the CC and PC groups of inde-

pendent observations have identical distributions ($p = 0.32$; two-sided Wilcoxon rank sum test). The same result holds for individual periods (the smallest p-value is 0.30 in the third period).

Figure 2.2 draws for each individual treatment boxplots of the average group contributions observed in each period, and illustrates the participants' tendency in the treatments allowing communication to contribute their entire endowment.[20] More specifically, in the CC treatment, 10 out of 18 groups are socially optimizing in periods 1 to 9 (one of them in periods 1 to 10). An equal number of groups are socially optimizing in PC in periods 1 to 9 (five of them in periods 1 to 10). In B_{10}, in contrast, two thirds of the groups never choose the socially efficient amount.

The aforementioned behavioral stability is corroborated by the results of Wilcoxon signed rank tests comparing the distributions of average group contributions in the first and ninth periods of each treatment. These tests detect no location shift different from zero in the case of CC and PC, but a significant period effect in the case of B_{10} ($p = 0.53$ for CC; $p = 0.36$ for PC; $p = 0.005$ for B_{10}; the reported significance levels correspond to the two-sided version of the test).

To conclude, the results of this section are consistent with our predictions and can be summarized as follows:

Result 2.1. *One-way communication significantly increases contributions to the public good and renders them relatively stable in all periods but the last.*

Result 2.2. *Whether the communicator can send a message prior to the first period only or prior to all periods bears no influence on contribution behavior.*

2.3.2 The Communication Content

Our categorization scheme of the communicators' arguments is described in Table 2.3 (the methodological details are given in Appendix A.1). The relative frequencies of observing the argument(s) implied by each category are reported in Table 2.4. To facilitate between-treatment comparisons, relative frequencies for CC are calculated separately for the first and then all subsequent periods.

[20] The boxplots corresponding to periods 2 to 9 (2 to 8) in CC (PC) collapse to a single value as all five statistics that they typically depict (lower non-outlier value, first quartile, median, third quartile, and higher non-outlier value) equal 25.

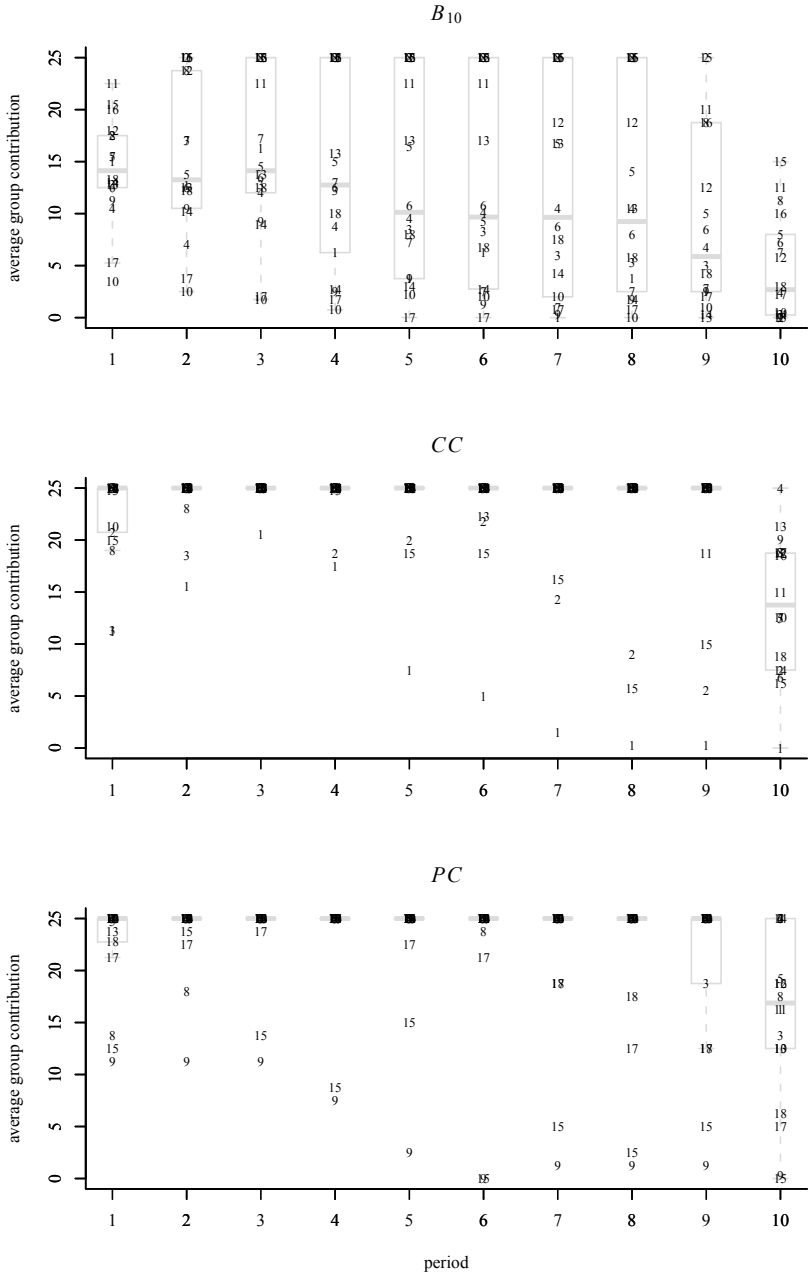


Figure 2.2: Average Group Contribution in all Periods (Finitely Repeated Treatments; the Observations are Represented by the Corresponding Group Numbers)

All first-period messages can be classified into at least one of our categories, which we interpret as a sign that the communicators took their task seriously. A comparison between the third and fourth columns of Table 2.4 reveals that the

Table 2.3: Description of the Communication Content Categories

Category	Argument	Description
1	Suggestion	Suggestion (point or interval) of how much to contribute to the project (period 1), or appeal to keep on with the same behavior (periods 2-10). The suggestion, whether implicitly or explicit, must be unambiguous.
2	Efficient suggestion	Implicit or explicit suggestion to contribute the whole endowment.
3	Conformity	Emphasis on the need that all group members conform to the suggestion.
4	Payoff calculation	Calculation of the (period or overall) payoff associated with the proposal.
5	Group payoff maximization	Explicit argument that the suggested amount maximizes the group payoff, or conjecture that participants are interested in maximizing the group payoff.
6	Satisfaction	Explicit argument that people should be content with following (or with having followed) the communicator's suggestion.
7	Fairness	Explicit reference to fairness or just behavior.
8	Team spirit	Statement promoting the willingness to cooperate as part of a team.
9	Promise	Pledge to contribute some specific amount.
10	Future repercussions of actions	Warning on the possible consequences of low contributions (e.g. "If one of us does not contribute, others may follow").
11	Notification of low contributors	Implicit or explicit notification of those who contributed less than suggested and/or request to increase their contribution.
12	Praise	Praise of observed actions.

Table 2.4: Relative Frequency of the Arguments' Presence in PC and CC

Category	Argument	PC	CC	
		$t = 1$	$t = 1$	$t > 1$
1	Suggestion	0.94	0.94	0.57
2	Efficient suggestion	0.83	0.78	0.51
3	Conformity	0.94	0.94	0.13
4	Payoff calculation	0.78	0.67	0.04
5	Group payoff maximization	0.50	0.78	0.01
6	Satisfaction	0.28	0.11	0.03
7	Fairness	0.22	0.17	0.02
8	Team spirit	0.44	0.28	0.09
9	Promise	0.00	0.11	0.03
10	Future repercussions of actions	0.06	0.17	0.01
11	Notification of low contributors	-	-	0.09
12	Praise	-	-	0.31

choice of first-period arguments is only marginally affected by the communication conditions. The vast majority of communicators propose a specific contribution (category 1), mainly the efficient one (category 2), and stress the importance of conformity within the group (category 3). In line with our conjectures, suggestion and conformity are always concurrent: the communicator seems to understand that if there are conditional cooperators in the group, contributing the suggested amount favors cooperation.

In both treatments, first-period suggestions are often accompanied by calculations of the associated payoffs (category 4). Communicators also try to motivate the others by drawing their attention to the payoffs that can be achieved under full cooperation (category 5). Arguments that rationalize suggestions on the grounds of either satisfaction (category 6) or fairness (category 7) are infrequent. The same applies to arguments that draw peoples' attention to the possible repercussions of their actions (category 10), in particular the likely effects of free riding on overall behavior.[21] Notice that unilateral promises (category 9) occur just twice in CC and never in PC.

In CC, the first-period messages in all ten groups that start with and retain (at

[21] Seely et al. (2005) show that "trigger" strategies, urging participants to reduce contributions after they experience defection, are generally not credible.

least till period 9) an average group contribution of 25 entail both the efficiency *and* the conformity argument.[22] These arguments do not appear in tandem in the first-period messages sent within groups numbered 2, 3, and 8, but each one of these groups achieves full contribution once its communicator has jointly invoked them. Hence, in CC, the conjunction of efficiency and conformity arguments seems to drive group contribution to the maximum.

In PC, the arguments of categories 2, 3, *and* 4 are mentioned in all groups where average group contribution remains fixed at 25 for (at least) the first nine periods.[23] In groups that do not consistently cooperate fully (that is groups 8, 9, 15 and 17), the communicators do not make an efficient suggestion and/or do not calculate the associated payoff.[24] Thus, in PC, the efficiency argument needs to be supplemented not only by conformity suggestions but also by payoff calculations in order that one-way communication has a strong impact on contribution levels.

The analysis of messages in the remaining periods of CC aims to answer two questions. The first is what communicators do in the face of initially high contribution levels. Our data indicate that once the efficient outcome has been achieved, the communicator sooner or later understands that group contribution will remain maximum even with minimal correspondence effort from his part. So most of the messages, if any, suggest to keep on with the same behavior (category 1), and/or praise past behavior (category 12). It is worth mentioning that the tenth-period messages in five out of the ten groups that socially optimize till period 9 explicitly point out that the interaction is coming to an end.[25] At least one member of each of these five groups free rides in the last period, possibly explaining the higher cooperation breakdown that we observed in CC, compared

[22] These are the groups numbered 4, 5, 6, 7, 9, 12, 14, 16, 17, and 18. The same holds for group 11 where average group contribution equals 25 for periods 1 to 8, and group 13 where deviations from maximum average contribution are (with the exception of period 6) no larger than 0.25 ECU.

[23] These are groups 1, 2, 4, 6, 7, 10, 11, 12, 14, and 16. The same happens with groups 5 and 13, where one group member deviates from maximum contribution in the first period.

[24] Here we provide a general description rather than exact rules. For example, even if the criteria of categories 2 to 4 are satisfied, the members of group 18 contribute fully just in periods 2 to 6. And the absence of payoff calculation does not prevent the members of group 3 to contribute fully in periods 1 to 8.

[25] These are groups 5, 7, 12, 14, and 16.

to PC.[26] The additional communication opportunities are, in this sense, harmful to cooperation.

The second question of interest is how communicators react to low contributors. In the three groups where first-period contributions are less than suggested, this is communicated to the other group members in the second period (category 11). In group 10, the communicator emphasizes the potential detrimental consequences of low contributions (category 10) and his threats prevent free riding in all but the final period. In group 1, the communicator's appeal to fairness (category 7) fails to stabilize contributions. Finally, the communicator of group 15 undercuts (in the first period) his own suggestion. The group achieves full contribution in the following three periods, but average contribution declines dramatically following a second attempt by the communicator to free ride.

2.4 Results on the One-Shot Treatments

We ran one session per treatment (B_1 and C). Each session involved 32 participants. Figure 2.3, panel A, draws histograms of the two data sets of individual contributions. While the distribution of the B_1 data is skewed to the right, that of the C data is bimodal (with more than 50% of the data points falling into the two extreme classes).

Figure 2.3, panel B, graphs empirical estimates of the cumulative distribution functions for the distributions that generated the two treatments' contribution data.[27] In the B_1 treatment, the function rises steeply for $c_i \leq 15$ (over 90% of the observations are less than or equal to 15) and levels off for the remaining values. In the C treatment, in contrast, less than 50% of the observations are lying within the $[0, 15]$ range. Following the introduction of one-way communication, the mean (median) contribution rises from 6.9 (5.0) to 14.6 (17.5). Formal testing, with group averages as independent observations,[28] confirms that the two underlying probability distributions are stochastically different (the p-value of the two

[26] Recall that 5 groups in PC, but only 1 in CC, contribute 100% of their endowment in period 10.

[27] The empirical cumulative distribution function $F(c_i)$ gives the proportion of observations in a sample which are less than or equal to c_i.

[28] Since members of a given group share exposure to the same message, which – we know – affects behavior, group contributions cannot be considered fully independent of each other.

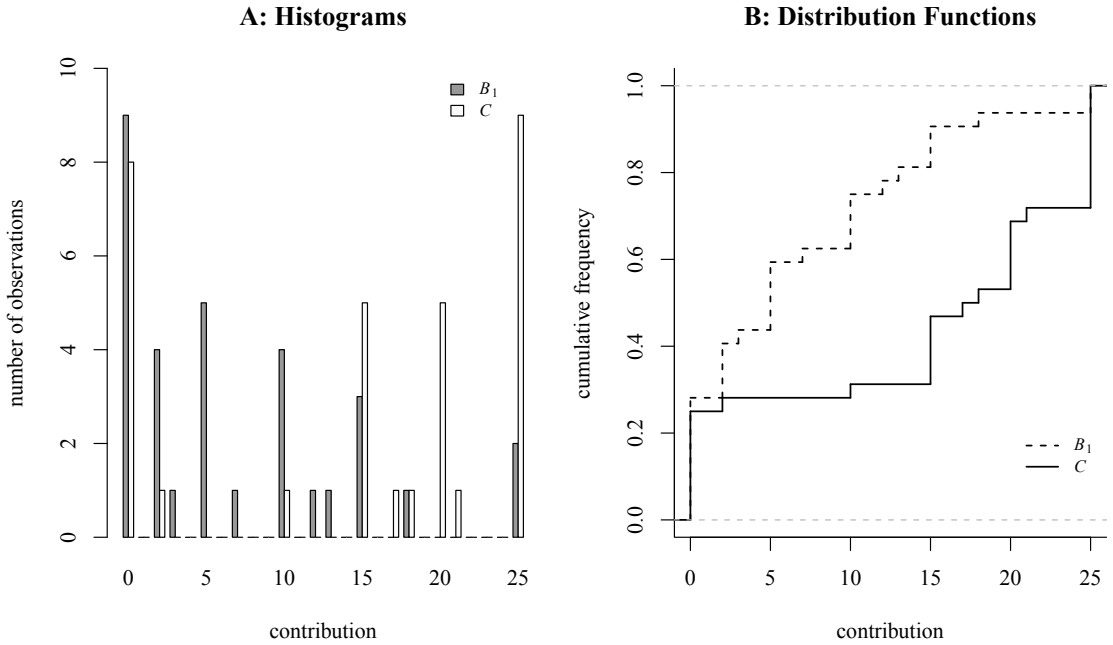


Figure 2.3: Histograms and Empirical Cumulative Distribution Functions of Individual Contributions in the One-shot Treatments

sample Kolmogorov-Smirnov test is less than 0.02), implying that strategic play can not be the driving engine of the effectiveness of one-way communication.[29]

Thus, the answer to the fourth question of Table 2.1 can be formulated as follows:

Result 2.3. *One-way communication stimulates contributions even when subjects are denied the possibility to play strategically.*

Finally, Table 2.5 reports the occurrence frequencies of the arguments that apply to one-shot communication.[30] Among all groups with an average group con-

[29] We perform the following analysis in order to compare the power of communication in the first period of the finitely repeated games and the one-shot games. We calculate the difference (Δ) between the average of the first period contributions in B_{10} and the average of the contributions in B_1. Δ is treated as a measure of the effect of strategic reasoning. We subtract it from the first period average group contributions in CC and PC, and use the Wilcoxon rank sum test to assess whether the distribution of the resulting sequences is identical to the distribution of the average group contributions in C. The obtained test statistics are not significant at conventional levels ($p = 0.26$ for CC vs. C and $p = 0.17$ for PC vs. C; two-sided tests), which we interpret as evidence of the independence of the communication effect from the type of interaction (i.e., one-shot vs. repeated).

[30] All messages can be classified into at least one category, attesting once again the communica-

Table 2.5: Relative Frequency of the Arguments' Presence in C

Category	Argument	Relative frequency
1	Suggestion	1.00
2	Efficient suggestion	0.75
3	Conformity	0.88
4	Payoff calculation	1.00
5	Group payoff maximization	0.50
6	Satisfaction	0.00
7	Fairness	0.12
8	Team spirit	0.62
9	Promise	0.38

tribution at least equal to 65% of the endowment (the groups numbered 1, 2, 3, 4, 7 and 8), all but two communicators (in groups 3 and 7) mention the arguments pertaining to categories 2, 3, and 4. The absence of the efficiency suggestion in the messages sent within groups 5 and 6 is associated with lower average group contributions. Unilateral promises are again infrequent: 3 out of the 8 communicators promise to contribute a specific amount. These results parallel our findings for the PC treatment, and confirm the importance of advancing the efficiency and conformity arguments, as well as of exemplifying payoff computations, to contribution levels.

2.5 Conclusions

Contrary to the widespread view that, in public goods games, the mutual exchange of promises is necessary for the cooperation-enhancing effect of communication, we find that one-way communication, in the form of leading by words, increases contributions significantly. Our interpretation of this result is that the communicator acts as an efficient coordination device, as preferences for conditional cooperation can transform the original game into a coordination game with multiple equilibria (see, e.g., Sen, 1967).

The rise in contributions does not depend on the frequency of communication and holds even if there are no prospects of future interactions. Our analysis

tors' commitment to reasoned arguments.

of the messages' content reveals that unilateral promises are rather infrequent. Most communicators successfully invoke the consequences of efficient behavior and stress the importance of conformity within the group: in the presence of conditional cooperators, the efficient outcome can be achieved only if all group members follow the suggestion of the leader. Thus, while decades of experimental research have shown that Hobbes was wrong in holding that "covenants without swords are nothing but words" (as people keep their promises even in the absence of sanctions), our experimental results suggest that, at least in the scenario that we considered, no covenants are needed in order to establish mutually beneficial cooperative relationships.

Understanding exactly how coordination contributes to the effectiveness of one-way communication is beyond the scope of this study and may provide a fruitful avenue for future research. Yet, the practical implications of our results are worth pointing out: a low-cost communication medium, like the internet, may be a suitable platform for addressing issues raised in social dilemma problems. Even with projects where many individuals interact via long distance, what seems to do the trick is the presence of a collaborator who sends a timely message to the others exhorting them to cooperate.

CHAPTER 3

A VOLUNTARY CONTRIBUTION EXPERIMENT WITH ONE-WAY COMMUNICATION AND ENDOWMENT ASYMMETRY*

*This chapter is based on the article "A voluntary contribution experiment with one-way communication and income heterogeneity" by Koukoumelis, Levati, and Weisser, forthcoming in Applied Economics Letters. The working paper version can be found in Koukoumelis et al., 2010, Jena Economic Research Papers 094. The authors thank Christoph Göring for his help in programming and conducting the experiments.

3.1 Introduction

According to standard economic theory, markets underprovide public goods owing to the free-rider problem. Experimental and field evidence may suggest otherwise, but the observed outcome is typically suboptimal (see, e.g., Ledyard, 1995). Since the provision of public goods influences the functioning and well-being of our societies, social scientists and policy makers aim at finding mechanisms that could propel individuals towards the social optimum.

In the context of any particular public goods production technology, the players' behavior depends on the choice of values for the various environmental and design variables. In Chapter 2, we show that one-way communication, or more specifically a free-form text message sent by one group member to his co-players before contribution decisions are made, enhances efficiency in linear voluntary contribution mechanisms (VCMs). In this chapter, we investigate the robustness of this communication method as a mechanism for the alleviation of the free-rider problem.

The experimental research on one-way communication has so far involved players that receive equal laboratory endowments. It could be that the efficiency-enhancing properties of one-way communication hinge upon the symmetry in endowments.

In fact, the experimental literature on VCMs provides evidence that asymmetry deters cooperation both when communication is not allowed (see, e.g., Cherry et al., 2005, Buckley and Croson, 2006) and when group members can communicate face-to-face (Isaac and Walker, 1988a).

In what follows we examine whether one-way communication is effective in fostering contributions in the presence of asymmetrically endowed players. Conditionally cooperative preferences (for a survey see Gächter, 2007) can transform the social dilemma game into a coordination game with multiple Pareto-ranked equilibria (see, e.g., Sen, 1967). Insofar as the communicator is able to draw the others' attention to an equal payoffs (rather than an equal contributions) rule, the above form of asymmetry should not be relevant to the effects of one-way communication.[1]

[1]That the communicator's cheap talk increases the amount of efficient play in coordination games is consistent with both theory and experimental evidence (Cooper et al., 1992, Farrell

3.2 Experimental Design

In order to facilitate comparison, the experimental design resembles that of the study presented in Chapter 2. Groups of size four interact for 10 periods in a partners design. At the beginning of every period t, each player i is endowed with $e_{i,t}$ ECU (Experimental Currency Units), which he can either consume privately or contribute to a public good. Individual endowments are asymmetric: there are two "rich" members endowed with 30 ECU and two "poor" members endowed with 20 ECU (the overall endowment is therefore 100 ECU per period, as in Chapter 2). The individual endowments remain constant throughout the game and are commonly known.

Let $c_{i,t}$ denote individual i's contribution to the public good in period t (with $0 \leq c_{i,t} \leq e_{i,t}$) and $C_t = \sum_{j=1}^{4} c_{j,t}$ be the provided public good. The period t monetary payoff of each i is given by

$$\pi_{i,t}(c_{i,t}, C_t) = (e_{i,t} - c_{i,t}) + 0.4\, C_t.$$

Since the marginal per capita return is less than unity, the dominant strategy for a monetary payoff maximizer is to contribute nothing. On the other hand, what is socially efficient is to contribute everything.

We study two treatments that build on the basic game described above. In the baseline treatment ($B_{10,A}$), group members cannot communicate with each other: in each period of a session, they decide simultaneously and privately on the number of ECU that they wish to contribute to the public good. In the communication treatment (CC_A), the game begins with one member of each group being randomly appointed communicator, a role that he retains throughout the experiment. Prior to each period's decision making, the communicator has a maximum of four minutes to compose a free-form message and send it to his co-players.

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted in the experimental laboratory of the Max Planck Institute of Economics (Jena, Germany). The subjects were undergraduate students from the Friedrich Schiller University Jena. Upon entering the laboratory, they were randomly assigned to visually isolated computer terminals. The instructions were distributed and

and Rabin, 1996, Crawford, 1998).

Table 3.1: Summary Statistics of Average Group Contributions

	Mean	Standard deviation	Median	Median absolute deviation
$B_{10,A}$	10.66	5.47	10.75	6.67
CC_A	19.73	7.47	25.00	0.00

Note: 160 observations per treatment.

then read aloud to establish public knowledge.[2] Before starting the experiment, subjects had to answer a control questionnaire testing their comprehension of the rules. In both treatments, all players received at the end of each period feedback on the number of ECU contributed by each group member, the income from the project (that is $0.4\,C_t$), and their corresponding payoff. We implemented an exchange rate of 10 ECU = 0.50 euro. The average earnings per subject were 19.62 euro (inclusive of a 2.50 euro show-up fee).

We ran two sessions per treatment. Each session involved 32 participants. With group size equal to four, we have 16 independent observations per treatment.

3.3 Experimental Results

Table 3.1 documents summary statistics for the average group contributions. The reported measures of location increase in response to the introduction of one-way communication. The variation, in contrast, decreases, provided that we acknowledge the presence of outliers and consider a robust measure of spread (like the median absolute deviation).

Figure 3.1 depicts the time paths of the means of the average group contributions. For the reader's convenience, we present as well the outcome of the symmetric-endowment treatments: $B_{10,S}$ (CC_S) corresponds to B_{10} (CC) in Chapter 2.[3] Treatment $B_{10,A}$ replicates standard findings: the mean of average group contributions starts above the series' overall mean and follows a downward trend. In contrast, the CC_A treatment mean of average group contributions

[2] All instructions can be found in Appendix B.2.

[3] The additional index S is meant to emphasize the symmetric parametrization of these treatments.

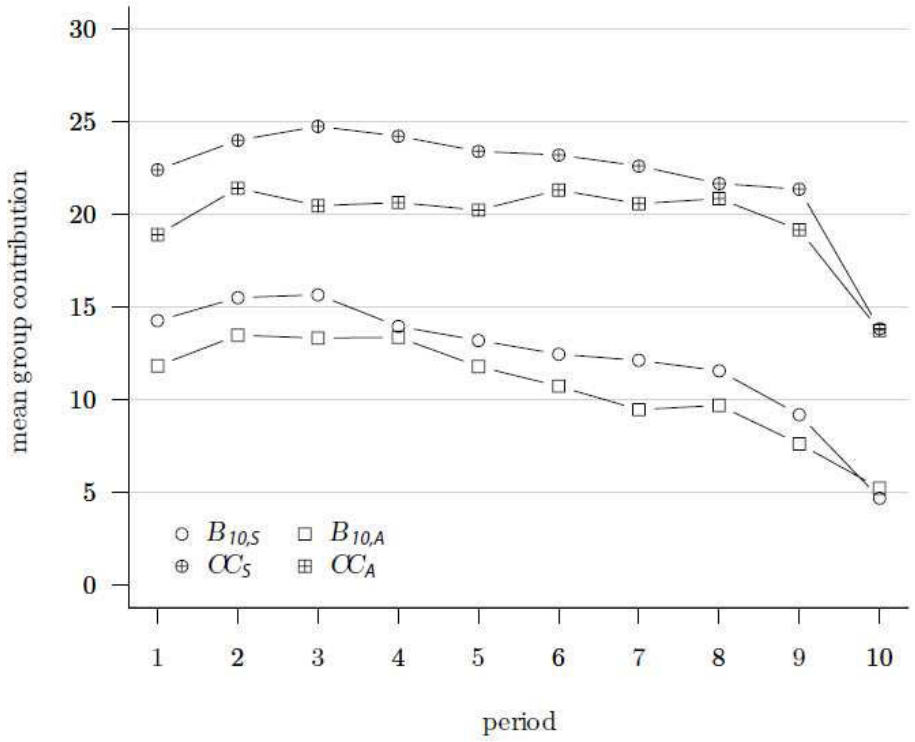


Figure 3.1: Mean of Average Group Contributions Over Time

starts at a notably higher level and remains fairly stable in all periods but the last. A one-sided Wilcoxon rank sum test (W) with mean group contributions averaged over all 10 periods as independent observations confirms that contributions in CC_A are significantly higher than contributions in $B_{10,A}$ ($p = 0.00$). In CC_A, the average group contributions of the groups that are led by rich communicators are not significantly different from those of the groups that are led by poor communicators ($p = 0.25$, two-sided W). These results are consistent with our main hypothesis on the efficacy of one-way communication. Additionally, contributions are higher in CC_S than in CC_A, and in $B_{10,S}$ than in $B_{10,A}$, but the differences are not significant ($p = 0.36$ and $p = 0.76$, respectively; two-sided W).

Figure 3.2 graphs the average relative contribution (that is contribution divided by endowment) of poor and rich subjects in all periods. In line with the results of previous experiments, in $B_{10,A}$ poor subjects contribute a larger share of their endowment than rich subjects do. Conversely, in CC_A the relative contributions of poor and rich are similar. Evidence to this is provided in Table 3.2

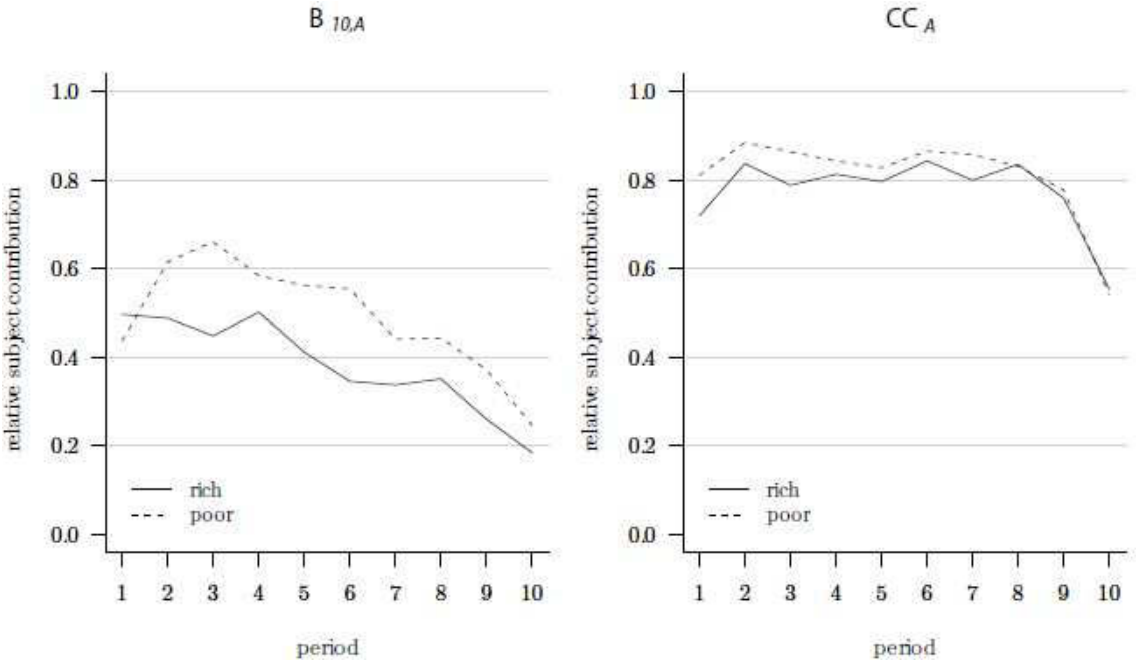


Figure 3.2: Average Relative Contributions of Poor and Rich Subjects Over Time

where we model the temporal pattern of $c_{i,t}/e_{i,t}$: the coefficient of the endowment dummy is significant in the $B_{10,A}$ regression but insignificant in the CC_A regression.[4] The communicator directs the players' attention away from an equal (in absolute terms) contributions rule, and successfully evokes a coordination rule prescribing contributions that equalize final payoffs.[5] In particular, 10 out of the 16 communicators suggest in their first period messages that all group members should contribute their whole endowment. Rich as well as poor subjects adhere to these suggestions in 80% of the cases.

3.4 Conclusions

Past studies dealt with one-way communication as a mechanism that fosters contributions in symmetric VCMs. This study examines a more realistic scenario. It demonstrates that the coordination role of the communicator is robust to situa-

[4]Table 3.2 presents parsimonious models that optimize the Bayesian information criterion. Yet, the results remain valid for alternative regression specifications (see Appendix B.1).

[5]In $B_{10,A}$ the median contribution of both poor and rich subjects equals 10, whereas in CC_A it equals 20 for the poor and 30 for the rich. See Van Dijk and Wilke (1995) and Van Dijk et al. (1999) for studies showing that participants in resource dilemmas prefer an equal payoffs rule.

Table 3.2: Random-Effects Tobit Regression Results for Relative Contributions to the Public Good

	$B_{10,A}$	CC_A
constant	0.352 (0.000)	-0.461 (0.105)
t	-0.035 (0.000)	0.322 (0.000)
t^2		-0.032 (0.000)
$d_{endowment}$	-0.168 (0.001)	-0.111 (0.501)
$\frac{\sum_{j=1}^{4} c_{j,t-1}/e_{j,t-1}}{4}$	0.807 (0.000)	1.790 (0.000)
Wald test	199.89 (0.000)	99.94 (0.000)
BIC	552.7	614.7

Note: The dependent variable is $c_{i,t}/e_{i,t}$ (576 observations grouped by subject). t stands for trend; $d_{endowment}$ equals 0 for the poor and 1 for the rich. Significance levels are reported in parentheses. The $B_{10,A}$ (CC_A) regression involves 91 (53) left censored and 74 (401) right censored observations. BIC stands for Bayesian information criterion.

tions where individuals are asymmetrically endowed. Our interpretation of this finding is that the communicator turns the people's attention away from an equal contributions rule. He promotes instead an equal payoffs rule, in particular the rule that is leading to the highest jointly attainable payoff.

CHAPTER 4

LEADING BY WORDS IN PRIVILEGED GROUPS*

*This chapter is based on the single-authored article "Leading by word in privileged groups," Jena Economic Research Papers 2011-066. This study benefited greatly from discussions with Oliver Kirchkamp, Werner Güth, Christoph Engel, Gary Bornstein, Vittoria Levati, Sebastian Krügel, Matthias Uhl, and all participants of the ESI and IMPRS seminars in Jena. Nadine Marmai, Jochen Bick, Tina Hilbig, and Evgenia Grishina provided valuable support in conducting the experiments.

4.1 Introduction

In public good provision, the dictate of monetary payoff maximization predicts inefficient outcomes for the collective (Olson, 1965). While decades of experimental research demonstrate the prevalence of social preferences (see, e.g., Fehr and Schmidt, 2006), a general willingness to withstand selfish acts is not sufficient to resolve the inefficient provision of public goods. Since individual actors' decisions are typically highly interdependent (see, e.g., Fischbacher et al., 2001, Fischbacher and Gächter, 2010), successful collective action also requires a high level of coordination.

In Chapters 2 and 3, we have shown that one-way communication, i.e., a free-form text message sent from one group member to his co-players before contribution decisions are made, can provide the means for successful coordination. The main facts that have emerged from these experiments are (i) that most messages entail specific suggestions for efficient contributions and (ii) that participants generally follow these suggestions. Such one-way communication is a useful simplification of real world situations in which communication is uni- rather than multidirectional. Speeches as a tool to encourage cooperation when formal authority is lacking are one example. It is therefore important to identify the circumstances which are conducive to the cooperation-enhancing effect of one-way communication as well as its underlying mechanisms.

This study reports on a one-shot experiment with asymmetric benefits from the public good. More specifically, we consider a situation in which the communicator, i.e., the sender of a message, is also the main beneficiary of cooperation. Apart from its intuitive appeal, this scenario might challenge the effectiveness of one-way communication if those who send messages rely on efficient contribution suggestions as witnessed in previous experiments. Arguably, such prompts are even more likely if communicators benefit disproportionally from cooperation. Compliance with such suggestions, however, might suffer for at least two reasons. One such reason is the inequality in payoffs associated with an efficient provision of the public good. Since compliance with any suggestion is voluntary, it is likely to depend on the desirability of the projected outcome. Inequality averse subjects (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000) might thus

be less likely to follow prompts to cooperate when the benefits from cooperation are asymmetric. Another reason is that compliance with contribution suggestions might depend on the recipients' perception of the sender's intent. Parallel to findings on direct reciprocity, i.e., responses to actions (see, e.g., Falk et al., 2008), intentional ambiguity might diminish cooperative responses to written prompts. While any message which encourages cooperation in a social dilemma might be perceived as selfish, this is arguably more likely if the sender also benefits disproportionally from the public good.

In his topology of groups, Olson (1965) identified the particular asymmetry in benefits from cooperation, which we described before, in what he called "privileged groups." Such collectives consist of two types of actors: high-benefit group members for whom the benefits from the public good outweigh the costs for providing it and low-benefit members, for whom the opposite is the case.[1] Olson's labeling already signifies the argument that, given actors care only about their own monetary outcome, privileged groups should enjoy higher quantities of the public good than normal, non-privileged groups since the latter uniquely consist of low-benefit members.[2] The only two experiments that explicitly examined privileged groups are not univocal with respect to this conjecture.[3] Glöckner et al. (2011) report that the presence of a high-benefit member inhibits the cooperation of low-benefit members, although the former makes efficient contributions. As a consequence, privileged and normal groups enjoy similar quantities of the public good. Reuben and Riedl (2009), in contrast, confirm Olson's conjecture as correct when punishment opportunities are unavailable. When sanctioning of (mis)behavior is allowed, however, privileged groups lose their status completely as low-benefit members largely refuse to react to punishment by high-benefit members. Reuben and Riedl (2009) thus demonstrate that the term "priv-

[1] Examples for privileged groups include the international efforts to fight terrorism, where primary targets such as the U.S. and the U.K. would still have an incentive to provide the funds needed even if all other country would refuse to assist. Scientific projects may share similar characteristics. Imagine, e.g., a graduate student who needs a co-authored study to be completed for his Ph.D.

[2] This argument is consistent with experimental evidence confirming that the marginal per capita return for providing the public good is a strong predictor of behavior (see, e.g., Marwell and Ames, 1979, Isaac and Walker, 1988b, Ledyard, 1995, Fisher et al., 1995).

[3] Palfrey and Prisbrey (1996, 1997) and Brandts and Schram (2001) allow for the "accidental" formation of privileged groups. Due to the information structure in these experiments, however, participants were not aware what kind of group was formed in a given period.

ileged" can be misleading when it comes to the effectiveness of measures against free riding.

This study reports on the first experiment comparing the effectiveness of one-way communication in normal vis-à-vis privileged groups.[4] The addition of baseline conditions without any opportunities to communicate also allows for insights with respect to the conjecture that privileged groups enjoy higher quantities of the public good. The results indicate no such advantage. On the contrary, low-benefit members contribute even less when matched with a high-benefit member, which reveals considerable inequality aversion. The effectiveness of one-way communication, however, is not affected by group composition. Even messages from senders who benefit disproportionally from cooperation elicit substantial increases in contributions from low-benefit members. This is also surprising because the post experimental questionnaire reveals that high-benefit members' messages are suspected of serving egoistic purposes.

What makes one-way communication effective? In Chapters 2 and 3, we emphasized the coordinative role of specific contribution suggestions. In this study, we test an alternative explanation which focuses on the basic fact that communication enables social interaction ex ante any binding decisions. This aspect is essential to some social psychological explanation attempts as to why (multi-way) communication enhances cooperation. Based on social identity theory (Tajfel and Turner, 1986), it is argued that communication may create affinity for the group and hence more cooperation (see, e.g., Orbell et al., 1988). Accordingly, well structured argumentation might not even be necessary for communication to strengthen contributions, and "giving someone a voice" might be sufficient. We test this conjecture for one-way communication. Specifically, we contrast a baseline with two communication conditions, only one of which allows for messages related to the experimental game. Our results show that only game-relevant messages may increase cooperation beyond the baseline condition. This finding is in accordance with results from previous studies (Dawes et al., 1977, Bouas and Komorita, 1996) which compared unrestricted multi-way communication to conditions that enabled discussion about predefined topics but not the dilemma

[4] In fact, we are not aware of any public goods experiment that simultaneously allows for communication and asymmetric benefits from cooperation.

situation itself.

The remainder of the study is organized as follows. Section 4.2 describes the experimental design and procedures. Qualitative hypotheses are discussed in Section 4.3. Section 4.4 reports on the experimental results regarding contribution decisions, the communication content, and the post experimental questionnaire. Section 4.5 summarizes the findings and concludes.

4.2 Experimental Design

4.2.1 The Basic Public Goods Game

In all treatments, participants interact for a single period in a linear voluntary contribution mechanism (see, e.g., Isaac et al., 1984). Every group consists of three members, $i = \{1, 2, 3\}$, each of whom is endowed with E=25 ECU (Experimental Currency Units). All group members simultaneously decide about their individual contributions, $c_i \in [0, 25]$, to the public good. Every ECU a decision maker keeps is worth 1 ECU to him. Additionally, he earns β_i ECU for every ECU he or any other group member contributes to the public good. We refer to β_i as individual i's marginal per capita return (MPCR) or marginal benefit (from cooperation). Individual i's payoff can be summarized by

$$\pi_i(\mathbf{c}) \quad = \quad 25 - c_i + \beta_i \times \sum_{j=1}^{3} c_j \quad , \tag{4.1}$$

where $\mathbf{c} = (c_1, c_2, c_3)$ and $\sum_{j=1}^{3} c_j$ represent the strategy profile and the sum of contribution in *i*'s group, respectively.

Usually, public goods games are parameterized such that they satisfy $0 < \beta_i < 1, \forall i$ and $\sum_{i=1}^{3} \beta_i > 1$. The first inequality implies that a monetary payoff maximizing decision maker has a dominant solution to contribute nothing since he incurs a net loss of $1 - \beta_i$ ECU for every ECU he contributes. However, due to the second inequality it is efficient to contribute the entire endowment in the sense that it maximizes the sum of payoffs in a group.

4.2.2 Experimental Treatments

The experiment follows a 2×3 between subjects design. For the first dimension we vary the composition of the groups by introducing two types of players. A *normal group* consists of three *low-benefit members*, whose individual marginal benefit from cooperation are equal to $\beta_L = 0.6$. *Privileged groups*, in contrast, comprise two *low-benefit* and one *high-benefit member*.[5] The latter's MPCR is set to $\beta_H = 1.6$. Classification into player types takes place at random. Note that this implementation of privileged groups mirrors that in Reuben and Riedl (2009) and is consistent with the definition in the sense of Olson (1965).[6]

For the second dimension we vary the available communication technology. The baseline (or *B*) treatment offers no opportunity to communicate. In two further conditions, we allow for one-way communication. We give one randomly chosen group member, i.e., the communicator, the opportunity to send a written message to his co-players prior to the contribution decisions.[7] In privileged groups, the communicator is also the high-benefit member. To ensure that messages are non-verifiable (and thus cheap talk), the communicators' contribution levels could not be identified. The two conditions allowing communication differ only with respect to the permissible communication content. While relevant one-way communication allows for messages that relate to the decision situation, irrelevant one-way communication does not.[8] The two treatments are abbreviated to *RC* and *IC*, respectively. Table 4.1 summarizes the experimental treatments.

4.2.3 Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted in June and July 2011 in the experimental laboratory of the Max Planck Insti-

[5] In the experiment, high- and low-benefit members were labeled A- and B-types, respectively.

[6] The only difference compared to Reuben and Riedl (2009) is that in their setup, the types' MPCRs are set to $\beta_L = 0.5$ and $\beta_H = 1.5$. This discrepancy is deliberate as we want to preserve the social dilemma character of the decision situation also for the subsets of two low-benefit members.

[7] In the following, we use the term "follower" to denote those group members who cannot communicate in treatments which allow for one-way communication.

[8] The labels *game-relevant* and *game-irrelevant* would be more appropriate as we do not mean to prejudge any treatment effects. The simplification is intended to serve readability and is in accordance with previous studies (Dawes et al., 1977, Bouas and Komorita, 1996).

Table 4.1: Experimental Treatments

Treatment	**Normal groups** *(3 low-benefit members)*	**Privileged groups** *(1 high-, 2 low-benefit members)*
B	no communication	no communication
RC	relevant communication, low-benefit communicator	relevant communication, high-benefit communicator
IC	irrelevant communication, low-benefit communicator	irrelevant communication, high-benefit communicator

tute of Economics in Jena, Germany. The participants were undergraduate students from the Friedrich Schiller University Jena.[9] They were recruited using the ORSEE system (Greiner, 2004). Upon arrival, participants were seated at visually separated computer terminals. The instructions were distributed and then read aloud to establish common knowledge.[10] The comprehension of the experimental rules was tested by means of a control questionnaire. Any questions were answered privately at the participants' seats.

In both treatments allowing communication, the communicator had up to four minutes to compose his message but was also able to finish ahead of time. The implementation of relevant and irrelevant communication differed only with respect to the set of restrictions to the otherwise free-form messages. In both cases, the communicator was neither allowed to violate anonymity, nor to promise side payments, nor to threaten the other group members with anything that might occur after the experiment. As for irrelevant communication, the messages were additionally restricted to statements not relating to the experiment.[11]

[9] None of the subjects had previously participated in a public goods game experiment with communication opportunities.

[10] All instructions can be found in Appendix C.3.

[11] Implementing irrelevant communication in this way has the shortcoming that communicators may try to circumvent the restriction, e.g., by using clever wording or metaphors. An alternative would be to record the communicators' messages before distributing the instructions. While the latter method precludes any reference to the specifics of the public goods game, it does not entirely prevent the communicator from relating his message to the experiment. More specifically, subjects experienced in experimental paradigms such as the trust game might expect cooperation to be an important element of the experiment. As communicators, they might then attempt to promote "full cooperation" even without knowledge of the game and its parametrization.

All messages were collected, screened by an experimenter, and delivered simultaneously. If any restrictions were violated, the message was blocked and the communicator was notified about his misconduct. Such an incident happened only once in a normal group in treatment IC. It was common knowledge that (a) the messages were cheap talk (i.e., costless, non-binding, and non-verifiable), (b) all group members received exactly the same message from the group's communicator, and (c) only after having read the communicator's message could the group members decide simultaneously on their individual contributions.

The experimental procedure included four practice periods in which participants were matched with computerized agents programmed to choose contributions randomly. In treatments RC and IC, the practice periods did not include the communication stage. After the experiment, all participants received information about the other group members' individual contributions. Before the feedback was delivered, we distributed an unincentivized questionnaire to elicit the subjects' identification with their group, first order action beliefs, and the followers' perception regarding the intention underlying the communicator's message.[12,13] The questionnaire items' exact wording can be found in Appendix C.1.

Sessions lasted 60 minutes on average. Payoffs were quoted in ECU, where 1 ECU = 20 euro cents. High- and low-benefit group members earned on average 16.60 and 10.10 euro, respectively, including a 2.50 euro show-up fee.

4.3 Hypotheses

4.3.1 No Communication – Baseline

Given preferences to maximize own monetary payoffs, group composition should not affect behavior since both types of players have dominant strategies. A high-benefit member should contribute his whole endowment since he earns a net benefit of $1.6 - 1 = 0.6$ ECU for every ECU he contributes. A low-benefit member, in contrast, is expected to contribute nothing to the public good. Decades

[12]Since our main focus was on contribution decisions, we followed the advice in Gächter and Renner (2010) and elicited beliefs without payment.

[13]Delivering feedback after the questionnaire precluded the possibility that the participants' answers were affected by statements about payoffs.

of experimental research, however, have rejected the latter prediction (see, e.g., Chaudhuri, 2011) and it is thus reasonable to expect some positive contributions also from low-benefit members.

If we relax our assumptions and allow for preferences for equitable monetary outcomes, the behavioral predictions change. Outcome-based models (see, e.g., Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000) take into account that the privileged member benefits disproportionally from cooperation within the group. In fact, the only uniform contribution decision by all group members which leads to strict equality in payoffs is when overall contributions equal zero. Strictly positive contributions from low-benefit types increase the inequality in payoffs between the decision maker and the high-benefit member by exactly the same amount since $\beta_H - \beta_L = 1$. Notice that by the same argument, it is exclusively the low-benefit group members' contributions which generate payoff discrepancies across player types. A low-benefit group member who is sensitive to the inequality in payoffs is thus predicted to contribute less in privileged than in normal groups.

In both, normal and privileged groups, a contribution always benefits the group more than it costs the contributor. The aggregate effect on overall payoffs, however, differs according to group composition. While a contribution of 1 ECU generates a total payoff of $3 \times 0.6 = 1.8$ ECU in normal groups, this figure amounts to $2 \times 0.6 + 1.6 = 2.8$ ECU in privileged groups. This discrepancy might lead to differences in behavior if participants' choices are guided by concerns for overall efficiency (see, e.g., Engelmann and Strobel, 2004). While a preference for overall efficiency coincides with monetary payoff maximization for high-benefit types, it counteracts the incentive to free ride for low-benefit members.

In summary, it can be said that high-benefit members are expected to contribute their whole endowment under monetary payoff maximization. Neither inequality aversion nor a preference for efficiency contradict this prediction. We propose our first hypothesis:

Hypothesis 4.1. *High-benefit group members contribute their entire endowment.*

Both, maximization of the own monetary payoff and preferences for equality in payoffs, predict lower contributions from low- than from high-benefit types, leading to Hypothesis 4.2:

Hypothesis 4.2. *High-benefit group members contribute more than low-benefit members.*

It is not straight-forward how the composition of groups will affect low-benefit types. Preferences for efficiency suggest higher contributions, while preferences for equality suggest lower contributions in privileged than in normal groups. While, strictly speaking, we are not in a position to propose a precise behavioral hypothesis ex ante, the observed behavior will help us to make an inference about the relative strength of the opposing effects ex post.

4.3.2 One-Way Communication

In social dilemma experiments, participants seize communication opportunities usually in an attempt to mitigate the free-rider problem. In our setup, however, high-benefit members are essentially unproblematic since they themselves have an incentive to provide the public good. As a consequence, neither relevant nor irrelevant one-way communication are expected to change the behavior of high-benefit members and thus we state:

Hypothesis 4.3. *High-benefit group members contribute their entire endowment irrespective of any communication opportunities.*

Since high-benefit members should remain unaffected, we exclusively concentrate on the low-benefit types when discussing the potential effects of one-way communication.

Relevant Communication

If we assume common knowledge of rationality and preferences to maximize monetary outcomes, then costless, non-binding, and non-verifiable messages are, of course, pure cheap talk. Consequently, they should not affect the behavior of low-benefit members. However, previous studies on (relevant) one-way communication in normal groups have rejected this prediction. Even for the case in which subjects interact for a single period, relevant one-way messages have a strong and positive effect on overall cooperation (see Chapter 2). Since our

setup is different from theirs only with respect to the parametrization of the public goods game, we expect to replicate the effect for normal groups. We formulate Hypothesis 4.4.

Hypothesis 4.4. *In normal groups, contributions are higher with relevant one-way communication than with no communication.*

In Chapters 2 and 3, we observe that the majority of messages contain specific contribution suggestions, which mostly point to full cooperation, and that these suggestions are followed by the majority of participants. We conjectured that messages function as a coordination device, which is plausible as, e.g., preferences for conditional cooperation can transform the social dilemma into a coordination game with multiple Pareto-ranked equilibria (see, e.g., Sen, 1967, Fehr and Fischbacher, 2002).[14] It is also consistent with a stream of literature in social psychology (Kelley and Thibaut, 1978, Kelley, 1979) which argues that the (experimentally) "given" payoff matrix might diverge from the "effective" matrix and that the latter is directly linked to behavior. Or, as the sociologist Peter Kollock (1998a, p. 193) put it: "There is, after all, no guarantee that subjects play an experimental game as intended by the researcher [...] ."

There is little reason to expect that high-benefit communicators will be less likely than low-benefit communicators to try and promote high cooperation. After all, they have an even greater interest to do so, as they benefit disproportionally from the public good. Whether they are similarly successful will depend on the desirability of high cooperation for low-benefit types in privileged groups. Assume, for instance, that a communicator suggests full cooperation. Unanimous compliance with such a suggestion would lead to higher overall payoffs in privileged than in normal groups. The distribution of payoffs, however, would be equal in normal and maximally unequal in privileged groups since the total surplus is solely earned by the high-benefit communicator. Thus, if low-benefit types strive for efficiency, their compliance with high contribution suggestions might be high also in privileged groups. Compliance and thus the cooperation-enhancing effect of one-way communication might seriously suffer, though, if low-benefit types dislike payoff inequalities.

[14] According to Fischbacher et al. (2001) conditional cooperation can either be viewed as a consequence of, e.g., fairness motives or as a preference in itself.

Contribution suggestions by communicators are not only cheap talk but may also exhibit some self-serving character. If a communicator believes (with positive probability) that high contribution suggestions will, on average, lead to higher contributions, he will always want to promote cooperation. While this is true for both types of groups, the self-serving character of contribution suggestions seems intuitively more salient in the case of asymmetric marginal benefits, where communicators are also the main beneficiaries of cooperative play. We know, e.g., from research on trust games, that intentions are crucial for reciprocal action (see, e.g., McCabe et al., 2003, Falk et al., 2008). We are not aware of a similar result which connects the effectiveness of communication to the perceived intention underlying the communication content. If such a relationship exists, however, a higher saliency of selfish intentions is likely to lead to lower compliance with suggestions to cooperate.

Depending on the desirability of efficiency or payoff equality and the sensitivity to perceived intentions, one-way communication, or, more precisely, specific contribution suggestions might be more or less effective in privileged vis-à-vis normal groups. We suspect that the net effect will be correlated with how behavior of low-benefit members compares across both types of groups. Lower cooperation in the presence of the high-benefit member, for instance, would signal inequality aversion, which should weaken the effectiveness of one-way communication.

Irrelevant Communication

It has been a long-standing conjecture in social psychology that communication affects cooperation in social dilemmas by manipulating the social environment (see, e.g., Orbell et al., 1988). The same may be true for one-way communication. By sending their message, communicators might create an environment in which participants perceive their group no longer as anonymous and randomly assembled but rather as a social entity bound together by fate.[15] Research in sociology and social psychology suggests that even arbitrary categorization can lead

[15] In a similar vein, Greiner et al. (forthcoming) show that one-way video messages by receivers affect behavior in a dictator game. This result demonstrates that communication may have an impact in the absence of strategic effects. It is not directly applicable to our setup, however, since, in their experiment, messages could relate to the experimental game.

to higher cooperation in social dilemmas (see, e.g., Simpson, 2006, Yamagishi and Mifune, 2008). Thus, communicators' messages might not have to relate to the experimental game for one-way communication to be efficiency-enhancing. "Giving one group member a voice" might be sufficient to elicit identification with the group and enhance cooperation. This line of reasoning is equally applicable to normal as well as privileged groups and suggests the following hypothesis.

Hypothesis 4.5. *Irrelevant one-way communication, i.e., messages unrelated to the experimental game, leads to higher cooperation than no communication.*

Previous empirical findings cast doubt on the validity of this hypothesis. In two separate experiments, Dawes et al. (1977) as well as Bouas and Komorita (1996) find that even irrelevant multi-way communication (i.e., communication among all group members on a topic other than the experiment) does not lead to higher cooperation rates than no communication at all.[16] If "giving everyone in a group a voice" does not suffice to promote cooperation, it is doubtful that giving only one person a voice will.

4.4 Results

We conducted one session per treatment with 30 participants each. The data analysis is based on 10 group averages per treatment since communication allows for correlated decisions within groups.[17] Consequently, data is also averaged by group for analysis on a more disaggregate level, i.e., for low-benefit members or followers. Unless stated otherwise, we will use two-sided Wilcoxon rank sum tests for comparisons across treatments and two-sided Wilcoxon signed rank tests to compare matched pairs within treatments.[18] We first investigate contribution decisions. Afterward, we advance to the analysis of the communication content and the data from the post experimental questionnaire.

[16] For experimental evidence on game-irrelevant communication in an ultimatum bargaining game, see Roth (1995).

[17] In case of irrelevant communication, we exclude the group for which the message was blocked from our entire analysis. All results are robust to its inclusion.

[18] The null hypothesis of a Wilcoxon rank sum test states that both independent samples are drawn from the same population. A Wilcoxon signed rank test requires for any observation in one sample a unique counterpart in the paired sample. It tests the null hypothesis that the difference in medians between the samples is equal to zero.

4.4.1 Contribution Decisions

Figure 4.1, panel a, depicts the average group contributions in normal groups for all three conditions. It shows that average contribution increase steeply when relevant one-way communication is allowed both with respect to the baseline and with respect to treatment IC. Irrelevant one-way messages, in contrast, are associated with lower overall cooperation than no communication at all. Non-parametric tests confirm this impression. While relevant messages lead to significantly higher contributions both with respect to the baseline ($p = 0.01$) and irrelevant messages ($p = 0.004$), the latter two conditions show no significant differences ($p = 0.44$). Moreover, the same result holds if we exclude the communicators' decisions ($p = 0.02$, B vs. RC; $p = 0.65$, B vs. IC; $p = 0.02$, RC vs. IC).

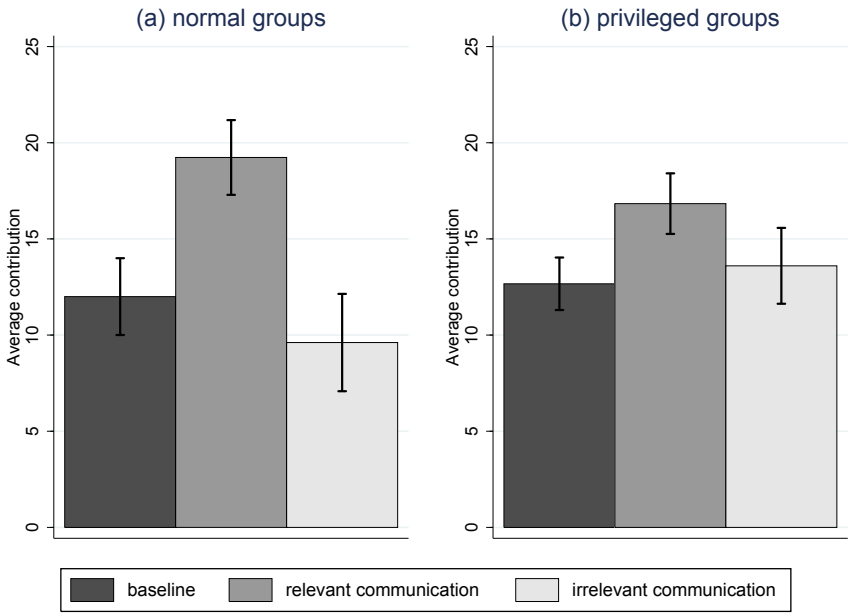


Figure 4.1: Average Group Contributions and 95% Confidence Intervals

The general picture for privileged groups, which is depicted in panel b of Figure 4.1, is qualitatively similar to that of normal groups. It is again relevant communication which elicits the highest average contributions. The absolute and relative advantage of one-way messages with respect to the baseline and treatment IC, however, seems smaller in privileged than in normal groups The null

hypothesis of equality in distributions is rejected on the 10% significance level only for the comparison between relevant communication and the baseline condition ($p = 0.053$). No other pairwise comparison reveals a significant difference ($p = 0.16$, RC vs. IC; $p = 0.82$, B vs. IC). We summarize with our first result.

Result 4.1. *We confirm that relevant one-way communication elicits higher average cooperation than no communication in a public goods game with normal groups. The effect also proves to be robust in privileged groups. The cooperation-enhancing effect of one-way communication is, however, confined to (game-)relevant messages. Merely "giving one group member a voice" does not suffice for one-way communication to increase cooperation. We do (do not) find that relevant messages lead to significantly higher contributions than irrelevant messages in normal (privileged) groups.*

In order to assess the alleged advantage of privileged over normal groups, we compare the contributions across the two panels of Figure 4.1. While average contributions are virtually identical in the two baseline conditions, contributions are higher in normal than in privileged groups when relevant communication is allowed. This relation is reversed when we consider irrelevant communication. Pairwise comparisons, however, fail to reject the null hypothesis of equality in distributions for all three conditions ($p = 0.97$ for B; $p = 0.18$ for RC; $p = 0.22$ for IC). We summarize with our second result.

Result 4.2. *Irrespective of whether we allow for no communication, relevant, or irrelevant one-way communication, we do not find support for the claim that contributions are higher in privileged than in normal groups.*

Both previous results have to be treated cautiously, as average group contributions may mask considerable heterogeneity in behavior across different player types. Figure 4.2 depicts type-specific contributions, where those of low-benefit members represent group averages.[19] Let us consider privileged groups. Hypothesis 4.3 holds that high-benefit members contribute their entire endowment irrespective of any communication opportunities. Low-benefit types are expected to contribute significantly less (see Hypothesis 4.2) but might be affected by one-way communication. Our data lend support to these conjectures. High-benefit

[19]The bars for normal groups are identical to those in panel a of Figure 4.1 since normal groups exclusively consist of low-benefit members.

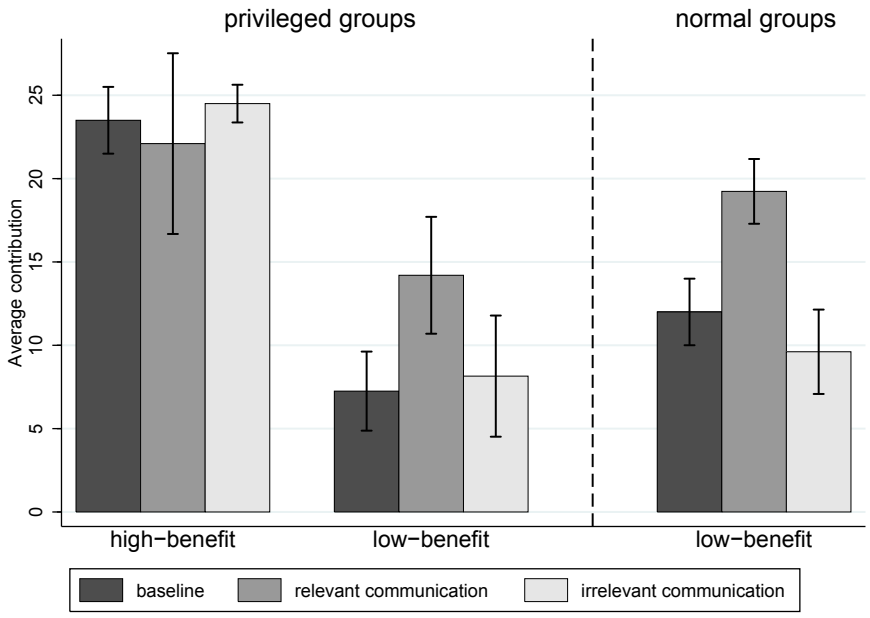


Figure 4.2: Type-Specific Contributions and 95% Confidence Intervals

members contribute, on average, between 88% and 98% of their endowment. In fact, 24 out of 30 individual observations are exactly equal to the entire endowment. Two-sided t-tests do not detect a location shift away from 25 ECU in any of the conditions ($p = 0.12$ for B; $p = 0.26$ for RC; $p = 0.34$ for IC). Furthermore, pairwise comparisons fail to reject the null hypothesis of equal distributions across conditions with and without communication opportunities (smallest p-value: $p = 0.28$ for RC). While high-benefit group members' contributions are very close to full efficiency, the contributions from low-benefit members fall short of that mark. On average, they contribute 29%, 56.8%, and 32.6% of their endowment in treatments B, RC, and IC, respectively. Comparing contributions between the different player types, the null hypothesis is rejected on the 1% significance level for both treatments B and IC (both $p = 0.005$) and on the 10% level for treatment RC ($p = 0.08$). Unlike high-benefit members' decisions, those of low-benefit members are sensitive to the availability of communication opportunities. This is already visible on the group level and becomes even more evident on the individual level, where the efficient contribution is observed only once in each treatment B and IC but nine times in treatment RC. Pairwise comparisons of low-benefit types' contributions reveal that relevant one-way messages lead to

higher cooperation than the baseline treatment, though irrelevant messages do not ($p = 0.04$, B vs. RC; $p = 0.97$, B vs. IC). As for relevant communication, contributions are marginally significantly higher than for irrelevant communication ($p = 0.095$). Figure 4.2 also suggests that relevant messages lead to similar (absolute) increases in low-benefit members' contributions in both types of groups. These findings qualify Result 4.1 and show that relevant one-way communication is effective in privileged groups, specifically as it leads to higher contributions by low-benefit members. We summarize with our third result.

Result 4.3. *In privileged groups, behavior is type-specific. High-benefit group members contribute almost their entire endowment irrespective of any communication opportunities. Low-benefit members contribute significantly less than high-benefit members, unless relevant one-way messages are possible. Relevant one-way communication affects low- but not high-benefit members' decisions. Irrelevant messages are never effective.*

Result 4.2 does not support the presupposed advantage for privileged groups but, according to Result 4.3, this is not due to the lack of contributions from high-benefit members. It is thus of interest to compare the behavior of low-benefit members across the different types of groups. Figure 4.2 shows that low-benefit members' contributions are lower in privileged groups for all three treatments. The discrepancy between privileged and normal groups is more pronounced in treatments B (4.7 ECU) and RC (5.0 ECU) than in treatment IC (1.5 ECU). Pairwise comparisons reveal that the difference is significant in the baseline treatment ($p = 0.03$), weakly significant with relevant messages ($p = 0.09$), and far from being significant with irrelevant messages ($p = 0.57$). We summarize with our fourth result.

Result 4.4. *Low-benefit members' contribution decisions are sensitive to the presence of the high-benefit member as contributions tend to be lower in privileged than in normal groups when communication opportunities are either absent or if relevant messages are allowed.*

This result deserves some discussion. First, note that Reuben and Riedl (2009) do not find that low-benefit members' decisions are sensitive to group composition. This discrepancy might be explained by their choice of a repeated partners

design which allows for reputation building. The opportunity to act strategically might induce low-benefit members to cooperate even if they dislike payoff inequalities. Glöckner et al. (2011) report that the presence of a high-benefit member deters cooperation of low-benefit members but only after a restart following ten initial periods. Our result is complementary as it shows that this effect does not rely on long sequences of repetitions. Result 4.4 is also informative with respect to the effectiveness of relevant one-way communication in privileged groups. It demonstrates that low-benefit types' contribution decisions are sensitive to payoff inequalities. We argue in Section 4.3 that such sensitivity could weaken the cooperation-enhancing effect of one-way communication in privileged groups. Results 4.1 and 4.3, however, show that relevant one-way messages are similarly effective in both types of groups. One possible explanation is that efficiency concerns, when voiced by the communicator, become increasingly important in the decision process. We discuss this in more detail in Section 4.4.2.

Results 4.1 through 4.3 show that relevant one-way communication yields an increase in contributions to the public good irrespective of group composition. But who benefits from this increase in cooperation? In order to answer this question, we investigate participants' payoffs, which are summarized in Table 4.2.

In normal groups, group-averaged payoffs are significantly higher in treatment RC than in the baseline and in treatment IC (both $p = 0.01$). The additional earnings benefit followers and, to a minor extent, also communicators. While followers in RC earn significantly more than both followers in IC and participants in the baseline treatment (both $p < 0.02$), the respective comparisons are only weakly significant ($p = 0.07$) and insignificant ($p = 0.18$) for communicators.

In privileged groups, the increase in average earnings associated with relevant one-way communication is less pronounced. In particular, average payoffs are only weakly significantly higher in treatment RC than in B and not significantly different for all other pairwise comparisons ($p = 0.054$, B vs. RC; $p = 0.82$, B vs. IC; $p = 0.16$, RC vs. IC). Since high-benefit members benefit disproportionally from the public good, it is not surprising that they earn significantly more than low-benefit members in all three treatments (all $p < 0.01$). A comparison between treatments, however, also reveals that high-benefit members are the only

Table 4.2: Payoffs by Treatment and Player Type

	Treatment	Average	Communicator / high-benefit	Follower / low-benefit
Normal groups	B	9.46	–	9.46
	RC	10.66	10.64	10.67
	IC	9.08	9.67	8.79
Privileged groups	B	12.09	14.99	10.64
	RC	13.61	19.27	10.78
	IC	12.42	15.67	10.79

Note: Payoffs are denoted in euro and include the 2.50 euro show-up fee.

beneficiaries of the opportunity to send (game-)relevant messages in privileged groups. While low-benefit members earn virtually the same in all three treatments (all $p > 0.12$), high-benefit members earn significantly more in treatment RC than in the baseline ($p = 0.04$).[20] In fact, while the average earnings differential between player types amounts to 4.35 and 4.88 euro in B and IC, respectively, it roughly doubles to 8.49 euro in treatment RC.[21]

4.4.2 Communication Content

We categorize the messages in treatment RC according to the scheme described in Table 4.3. All methodological details can be found in Appendix C.2. Table 4.4 reports the categories' relative frequencies of appearance in the messages' argumentation.

On average, a message entails arguments according to 4.8 and 4.2 categories in normal and privileged groups, respectively. We treat this as a sign that most communicators took their task seriously, trying to make an impact with their messages. In many respects, the messages are very similar in both types of groups. In normal as well as privileged groups, the majority of messages incorporates suggestions for specific (category 1) and efficient (category 2) contributions cou-

[20] No other pairwise comparison for high-benefit members reveals significant differences ($p = 0.93$, B vs. IC; $p = 0.11$, RC vs. IC).

[21] The earnings differential between high- and low-benefit members is significantly larger in RC than in B ($p = 0.04$), weakly significantly so in RC than in IC ($p = 0.096$), and not different for B and IC ($p = 0.97$).

pled with an emphasis on the importance of conformity within the group (category 3).[22] Statements pointing to fairness (category 7) or team spirit (category 8), which are most likely meant as auxiliary arguments to substantiate the contribution suggestions, are less frequent but present for both types of groups.[23] Half of all communicators signal their contribution intentions (category 9).[24] Most of them specifically promise to contribute their entire endowment. Note that such a promise is credible in privileged groups, as communicators are high-benefit members, but not in normal groups.

Apart from these similarities, there are some noticeable differences in the messages' content between the different types of groups. In fact, comparing the distributions of arguments between types of groups, a one-sided Fisher exact test gives some indication that the distribution of category appearance depends on group composition ($p = 0.07$). Most strikingly, we observe payoff calculations for seven out of ten normal groups but only for one privileged group. This difference is significant according to a two-sided Fisher exact test ($p = 0.02$). One rationalization for this observation might be that payoff calculations facilitate the demonstration of possible gains from cooperation but, at the same time, may also highlight the salience of payoff inequalities. High-benefit communicators might thus avoid computations altogether if they expect low-benefit types to be inequality averse. Instead, they might try to point out the possibility of efficiency gains in different ways, which is what we observe. Six high-benefit communicators mention group payoff maximization (category 5) – twice as many as in normal groups. This difference is, however, not significant ($p = 0.37$, two-sided Fisher exact test).

We have not categorized messages in treatment IC since they tend to be very heterogenous in their content. Many of them include only one "argument," if any. Three messages, for instance, merely incorporate the wish that the other group members may enjoy the experiment. Other messages just consist of nurs-

[22] While this is in accordance with the results for treatment C in Chapter 2, note that the relative frequency of suggestions is somewhat lower in the present study.

[23] Statements which relate to satisfaction (category 6) are absent in normal as well as privileged groups. We incorporate this category in order to facilitate comparison with the results in previous chapters.

[24] Note that the 40% promises in normal groups mirror the relative frequency of pledges in treatment C in Chapter 2.

Table 4.3: Description of the Communication Content Categories

Category	Argument	Description
1	Suggestion	Suggestion (point or interval) of how much to contribute to the project. The suggestion, whether implicitly or explicit, must be unambiguous.
2	Efficient suggestion	Implicit or explicit suggestion to contribute the whole endowment.
3	Conformity	Emphasis on the need that all group members conform to the suggestion.
4	Payoff calculation	Calculation of the payoff associated with the suggestion.
5	Group payoff maximization	Explicit argument that the suggested amount maximizes the group payoff, or conjecture that participants are interested in maximizing the group payoff.
6	Satisfaction	Explicit argument that people should be content with following the communicator's suggestion.
7	Fairness	Explicit reference to fairness or just behavior.
8	Team spirit	Statement promoting the willingness to cooperate as part of a team.
9	Promise	Pledge to contribute some specific amount.

Table 4.4: Relative Frequency of the Arguments' Presence with Relevant Communication

Category	Argument	Relative frequency	
		Normal groups	Privileged groups
1	Suggestion	0.8	0.7
2	Efficient suggestion	0.7	0.6
3	Conformity	0.9	0.6
4	Payoff calculation	0.7	0.1
5	Group payoff maximization	0.3	0.6
6	Satisfaction	0	0
7	Fairness	0.3	0.1
8	Team spirit	0.3	0.4
9	Promise	0.4	0.6

Note: The relative frequencies are based on 10 observations for each type of group.

ery rhymes. Four messages are seemingly intended to entertain the receivers as they contain a joke or humorous summary of the daily news. The only message that was blocked referred to the upcoming contribution decision and the possible payoffs.[25] Perhaps most rich in content, six messages relate to general fairness ideas ("love your neighbor as yourself," "harm set, harm get") or invoke solidarity principles (three senders mention the slogan of Dumas' musketeers "one for all, all for one").

4.4.3 Post Experimental Questionnaire

Group Identification

Some studies in social psychology argue that the effects of communication are driven by enhanced group identification. If this was the case (and given our results in Section 4.4.1), we should expect group members to identify more with their group in treatment RC than in the baseline. The ineffectiveness of irrele-

[25] We observe only one obvious attempt to circumvent the restrictions and still hint to to some desired contribution level. The sender of this message fabricates a story and mentions the number 25 wherever possible, but he never relates to the experiment. The average contribution from low-benefit members in that particular privileged group is 7.5 ECU.

vant one-way communication, in turn, might be explained by the lack of such an increase in identification. The data from the post experimental questionnaire permit us to investigate these conjectures. We calculate a participant's mean identification score as his average response to all four items. Comparing group averages across treatments, we find for normal groups that identification is indeed weakly significantly higher in RC than in B ($p = 0.06$) and not significantly different otherwise ($p = 0.97$, B vs. IC; $p = 0.12$, RC vs. IC).[26] The data from normal groups are thus not inconsistent with previous conjectures.

The picture changes when we consider privileged groups. While, on the group level, mean identification scores are weakly significantly higher in RC than in the baseline ($p = 0.06$), this is not the case for low-benefit members ($p = 0.21$). Since it is precisely the low-benefit members who react to relevant communication in privileged groups, the latter observation is inconsistent with the claim that communication works via enhancing group identification. A further puzzling observation is that irrelevant one-way messages lead to significantly higher identification than the baseline for group averages as well as for averages of low-benefit members ($p = 0.02$ and $p = 0.03$, respectively).[27] If communication was to affect cooperation via group identification, we should observe higher contributions in IC than in the baseline for privileged groups. The fact that we do not casts doubt on the conjectured link between (higher) identification and enhanced cooperation. It is thus also possible that the increased identification in treatment RC for normal groups is a by-product rather the driving force behind the effectiveness of relevant one-way messages.

Message Perception

In treatment RC, the majority of communicators makes specific contribution suggestions. Although these may always be interpreted as serving the communicator's monetary interest, we conjectured in Section 4.3 that such an interpretation was more likely in privileged groups, where the incentive structure highlights the communicator's interest in the public good. We also surmised that the perception

[26] This result proves robust if we focus on group averages of non-communicators ($p = 0.096$, B vs. RC; $p = 0.73$, B vs. IC; $p = 0.32$, RC vs. IC).

[27] In privileged groups, identification is not different for treatments RC and IC, neither for group averages nor for averages of low-benefit members.

that a suggestion serves a egoistic purpose might reduce the rate of compliance. Result 4.3 shows, however, that relevant messages increase the cooperation of low-benefit members also in privileged groups. The question arises whether this is due to the fact that followers are unaware of the messages' potentially egoistic character or that this property is unimportant for their decisions.

The questionnaire data (cf. item 9, Appendix C.1) might help to shed light on this question. Followers had to rate their degree of consent to a statement portraying the message they received as intended to maximize the communicator's own payoff. The data show that followers (averaged by group) have a significantly higher tendency to agree with this statement in privileged than in normal groups ($p = 0.001$).[28] Since it is not the lack of awareness, followers' choices must be insensitive with respect to perceived selfish purpose of messages. And indeed, the correlation between average follower contributions and their average responses to this questionnaire item turns out to be virtually zero ($\rho = 0.02$, $p = 0.97$). This insight is somewhat surprising as we know that perceived intentions are a crucial determinant of reciprocity (Falk et al., 2008). Despite the obvious discrepancy that the latter result deals with reactions to actions and not written statements, both situations share a seemingly crucial aspect. The sum and distribution of payoffs always depend on some agents' willingness to react to another agent's attempt to stimulate cooperation.

Expected Contributions

In Section 4.3 and in previous chapters, we have argued that relevant one-way messages, and, more specifically, contribution suggestions may serve as a coordination device. This concept assigns a critical role to expectations as suggestions supposedly affect prior beliefs about what to expect from other group members. Conditional cooperators should be sensitive to such changes in expectations. The questionnaire data reported in Table 4.5 provide some support for these conjectures.[29] On the group level, average expectations proof to be significantly higher in treatment RC than in the baseline for both types of groups (both $p = 0.01$). Irrelevant messages, on the other hand, do not significantly affect av-

[28] This is not the case for irrelevant messages ($p = 0.73$).
[29] Cf. items 5 to 8 in Appendix C.1.

Table 4.5: Expected Contributions by Treatment and Player Type

	Treatment	Average	Communicator / high-benefit	Follower / low-benefit	
				about: high-benefit	about: low-benefit
Normal groups	B	12.4	–	–	12.4
	RC	18.1	16.2	–	19.1
	IC	11	8.2	–	12.4
Privileged groups	B	12.1	9.5	19.3	7.5
	RC	15.4	11.4	23.6	11.1
	IC	13.4	11.2	21.4	7.6

Note: In order to obtain one (average) measure per privileged group, we first calculate a low-benefit member's average expectation as the mean of what he expects from both his co-players. Then, we average the expectations of all three group members.

erage expectations compared to the baseline treatment ($p > 0.41$, for both types of groups).[30] Interestingly, we do not find significant differences between expectations of communicators in treatment RC and those of average participants in the baseline treatment ($p = 0.59$) for normal groups. The same holds for high-benefit members' expectations in treatments RC and B ($p = 0.29$).[31] This result is remarkable as it indicates that communicators in treatment RC did not believe that their messages would enhance cooperation. Instead, it is followers who trust in the effectiveness of relevant one-way messages. In normal groups, they report significantly higher expectations in treatment RC than the average member in the baseline and followers in treatment IC (both $p < 0.01$). In privileged groups, this tendency is less marked as low-benefit members' expectations about the other low-benefit member's action are only weakly significantly higher in treatment RC than in the baseline and not significantly different otherwise ($p = 0.095$, B vs. RC; $p = 0.88$, B vs. IC; $p = 0.12$, RC vs. IC). Surprisingly, however, low-benefit members' expectations about high-benefit contributions are affected by relevant

[30] Relevant messages elicit significantly higher average expectations than irrelevant ones only in normal but not in privileged groups ($p = 0.01$ and $p = 0.16$, respectively).

[31] Communicators of normal groups report significantly lower expectations in treatment IC than communicators in RC or the average member in the baseline ($p = 0.02$ and $p = 0.048$, respectively). There are no significant differences in expectations for high-benefit members for any combination of treatments ($p > 0.29$, for all cases).

messages ($p = 0.01$).[32] This indicates that enhanced cooperation of low-benefit members in treatment RC might partially be due to a conditionally cooperative reaction to a falsely assumed (cf. Result 4.3) increase in contributions from high-benefit members. In summary, the belief data suggest that relevant but not irrelevant one-way communication generates more optimistic expectations about others' contributions. Most astonishingly, this effect is exclusively visible for followers.

4.5 Conclusions

In this study, we use a one-shot public goods game to investigate contribution behavior in privileged vis-à-vis normal groups with and without one-way communication. The contribution to the literature is three-fold.

First, we provide evidence that contribution behavior can be inversely related to other group members' marginal benefits from the public good when interaction is one-shot. While efficiency concerns would predict the opposite, the result is in accordance with fairness theories (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000) which account for payoff inequalities resulting from asymmetric marginal benefits. The lack of personal sacrifice in contributions from high-benefit members constitutes an alternative explanation (Glöckner et al., 2011). If personal sacrifice is defined dichotomously, a future experiment might succeed in distinguishing these two explanations by comparing normal and privileged groups to intermediate groups, in which marginal benefits are asymmetric but the dominance of free riding is preserved. Our results have implications for the alleged advantage of privileged groups (Olson, 1965). They show that the underprovision of the public good in privileged compared to normal groups depends on two opposing effects. The first effect is that high-benefit members contribute higher amounts than low-benefit members in normal groups. The other is that the presence of a high-benefit member inhibits cooperation by low-benefit members. Which of these effects dominates might depend on group composition. When they have many low- and few high-benefit members, privileged groups might

[32] Separate t-tests reject the null hypothesis that followers expected high-benefit members to contribute the entire endowment for all three treatments ($p < 0.02$, for all cases).

even enjoy lower quantities of the public good than normal groups.

Second, we confirm that one-way communication yields enhanced cooperation even if a sender of the message benefits disproportionally from the public good. Followers comply with communicators' persuasion attempts (i) despite their awareness that these might be based on self-serving intentions rather than a desire to benefit the group and (ii) despite the fact that such behavior inevitably yields unfavorable payoff inequality for themselves. The first aspect suggests that, in contrast to reciprocity with respect to actions, compliance with contribution suggestions does not depend on perceived intentions. The second aspect indicates that followers accept payoff inequalities more readily when one-way communication is available. One possible explanation for this observation is that communicators' arguments increase the weight of efficiency considerations in the decision making process. Overall, our experimental evidence makes a compelling case for the robustness of the effectiveness of one-way communication. This property should not be taken for granted. Reuben and Riedl (2009) have already shown that even the power of punishment opportunities tumbles in privileged groups.

Third, we test and reject the conjecture that one-way communication affects cooperation merely by "giving someone a voice." Messages which are restricted to topics other than the experiment do not affect contribution decisions irrespective of group composition. This result suggests that the mechanism underlying the effectiveness of one-way messages resides within the communication content.

CHAPTER 5

LEADING BY EXAMPLE IN INTERGROUP COMPETITION: AN EXPERIMENTAL APPROACH*

*This chapter is based on the single-authored article "Leading by example in intergroup competition: An experimental approach," Jena Economic Research Papers 2011-067. This study benefited greatly from discussions with Oliver Kirchkamp, Werner Güth, Christoph Engel, Vittoria Levati, Ro'i Zultan, Matteo Ploner, Sebastian Krügel, Matthias Uhl, and all participants of the ESI and IMPRS seminars in Jena. Adrian Liebtrau provided valuable assistance in programming and conducting the experiments.

5.1 Introduction

Effective leadership is an important element in modern organizations. It serves as a means to motivate effort from personnel in circumstances of incomplete contracts or where formal authority is lacking. Leading by example is, perhaps, the most basic form of leadership. It comes in many forms: the CEO working for a symbolic salary of 1$ when his company demands pay cuts from its employees, the foreman being the first on the site and the last to leave, or union leaders joining the street protests. Historical examples outside for-profit organizations include Martin Luther King's participation in the freedom marches or Joseph Stalin's decision to stay in Moscow during World War II (Hermalin, 1998).

This study reports on an experiment in which we extend the research on leading by example to a situation relevant to many organizations: a scenario of intergroup conflict within the firm. Intergroup conflict occurs when the interests of two or more groups are in opposition. This readily translates to the organizational context as groups, like departments or work teams, compete over scarce resources imposed by limitations in space, budget, or labor supply. The notion of group conflict is often exclusively associated with detrimental consequences which may arise as rivals have the incentive to waste resources for conflict-related activities. It may, however, also be used to the benefit of the organization (de Dreu and van de Vliert, 1997).[1] Examples include Oppenheimer's use of competing groups in the Manhattan Project (Gosling, 1999) and within-firm R&D competitions, e.g., at Samsung (Chen and Li, 2007).

The experimental literature examines leading by example in the environment of linear public goods games – an experimental paradigm, which is often used to study team work. Leading by example is implemented via a semi-sequential decision protocol. The leader acts as the first mover. His decision is revealed to the remaining group members who then decide simultaneously on their contributions. The experimental literature on leading by example includes, among others, Moxnes and van der Heijden (2003), Güth et al. (2007), and Levati et al. (2007). These studies generally show that leaders are willing to give good examples. Fol-

[1] The term intergroup competition seems to capture this notion more convincingly than intergroup conflict. The literature, however, treats these terms as interchangeable and it does not seem possible to act upon this issue in the present study.

lowers, however, react only partially and undercut the leaders' contributions. As a result, leading by example only weakly increases overall contributions to the public good. The basic paradigm of leading by example has been extended to incorporate different forms of heterogeneity (Levati et al., 2007, Glöckner et al., 2011), endogenous leader selection (Güth et al., 2007, Rivas and Sutter, 2008), or voluntary leadership (Haigner and Wakolbinger, 2010, Rivas and Sutter, 2011). None of the previous studies, however, dealt with a situation involving more than one group.[2]

Intergroup competition has received attention in a number of disciplines. For an excellent review on the experimental research in social psychology, see Bornstein (2003). A recent laboratory study in evolutionary biology is Puurtinen and Mappes (2008). Experimental studies in economics include, among others, Nalbantian and Schotter (1997), Gunnthorsdottir and Rapoport (2006), Tan and Bolle (2007), and Abbink et al. (2010). The evidence from all disciplines shows that intergroup competition can lead to an increase in intragroup cooperation in a large set of circumstances. The underlying mechanisms root in both strategic as well as motivational sources. They relate to in-group favoritism and social preferences – concepts closely linked to reciprocity which most likely is crucial to leading by example. This will be discussed in greater detail in Section 5.2.

We complement the existing literature in three ways. First, we generalize the paradigm of leading by example to a scenario of intergroup competition. We examine both one-shot and repeated interaction. The former abstracts from strategic considerations and allows us to investigate leader and follower behavior in a clean environment. The latter accounts for the fact that real life intergroup conflict mostly entails multiple encounters. Second, we use the strategy method when eliciting followers' decisions. This enables us to fully characterize followers' types and to examine the change in conditional follower responses when intergroup conflict is introduced.[3] Third, we elicit group identification and analyze its relation to the effect of intergroup competition.

[2]For theoretical and experimental work on leading by example with information asymmetries between leaders and followers, see Hermalin (1998) and Potters et al. (2007).

[3]Gächter et al. (2010) also elicits followers' choices via the strategy method, but in a two-person game. In such a setup, a follower's choice does no longer entail any behavioral uncertainty, which is present in our design. Their measurement is thus more closely related to the elicitation of conditional cooperative attitudes (see, e.g., Fischbacher et al., 2001).

Our results for the one-shot interactions show that intergroup competition has differential effects on leader and follower behavior. While leaders are largely insensitive to the presence of intergroup conflict, followers display an increased willingness to cooperate. This increase does not depend on the leaders' actions. When groups interact repeatedly, we do not find that leading be example is able to foster cooperation by itself. It only significantly improves contributions when it is accompanied by intergroup competition. Our data do not support the conjecture that intergroup competition leads to higher group identification.

The chapter proceeds as follows. Section 5.2 illustrates our hypotheses. Section 5.3 describes the experimental design, Section 5.4 presents the results, and Section 5.5 concludes.

5.2 Theoretical Considerations and Hypotheses

Embedding a social dilemma in an intergroup conflict affects individuals' decisions in two important ways. First, it changes incentives. If groups enter a competition for an exogenously given price as in winner-takes-all or rent-seeking (or Tullock) contests, cooperation becomes more profitable because it increases the chances for winning the prize (see, e.g., Bornstein et al., 1990, Abbink et al., 2010).

Intergroup conflict has a second and purely motivational effect on intragroup cooperation and we are exclusively interested in the latter.[4] Its existence has been a long-standing conjecture in social psychology (see, e.g., Messick and Brewer, 1983, Brown, 1988). Yet, Bornstein and Ben-Yossef (1994) were the first to provide a clean experimental test. They designed an experiment to compare behavior in a single group prisoner's dilemma (PD) and in an intergroup prisoner's dilemma game (IPD). Both games were identical with respect to their intragroup social dilemma structure. The IPD, however, models two competing groups, where cooperation in any one of them inflicts a negative external effect on the respective opponent. Bornstein and Ben-Yossef (1994) report twice as much cooperation in the IPD than in the PD. Since both games were identical with respect to ma-

[4] In fact, our experiment is expressly designed to control from the afore mentioned incentive effect.

terial incentives of individuals and groups, the authors attributed the effect to purely motivational reasons. More specifically, they state that the difference in behavior "[...] cannot be explained by assuming that subjects were motivated by self-interest, group-interest, or some fixed combination of both." (p. 64). Since, in the IPD, a cooperative act benefits the own group and hurts the out-group at the same time, a greater concern for the in-group's outcome or spite toward the out-group remained possible explanations. Halevy et al. (2008) presents evidence on this distinction. In their experiment participants have the choice whether their cooperative act shall decrease the out-group's outcome in addition to increasing the in-group's. The results show that the vast majority of participants chooses not to hurt the out-group. In conjunction with the results in Bornstein and Ben-Yossef (1994) this evidence strongly suggests that intergroup competition leads to enhanced concerns for the in-group's overall outcome.

We implement intergroup competition in the same way as Bornstein and Ben-Yossef (1994) who examine pure one-shot interaction. It is therefore reasonable to expect similar results in our one-shot encounters. It is more difficult to foresee behavior in the repeated interaction setup as empirical results are mixed. Bornstein et al. (1996), e.g., find that the motivational effect of intergroup competition diminishes with repetition. We state our first hypothesis:

Hypothesis 5.1. *When interaction is one-shot, intergroup competition leads to an increase in intragroup cooperation.*

The motivational effect associated with intergroup competition might impact leading by example. First, we expect that leaders contribute more to the public good if they assign greater weight to the outcome of their group. Since we know from previous experiments that followers' contribution decisions are positively correlated with those of leaders, such a behavioral change would yield higher overall cooperation. Followers, in turn, might be willing to reciprocate a leader's contribution more forcefully given that the group's outcome figures more prominent for their decisions. Such a behavioral shift would counteract the followers' general tendency to undercut leaders' contributions, which previous studies identified as the most serious obstacle for leading by example to effectively increase cooperation.

An increased willingness to reciprocate leaders' examples is conceivable in different forms. One possibility is that the enhanced cooperation in intergroup competition is associated with a greater concern for the in-group's success which is not conditional on the decisions of other group members. In this case, we would expect that followers increase their contributions to every possible leader decision by some fixed amount. An alternative would be that intergroup competition leads to an increased willingness to cooperate that is conditional on the other group members' readiness to forego their individual monetary interest as well. In other words, intergroup competition might lead to a stonger tendency for conditional cooperation. In this case, we would expect that followers' marginal responses to increases in leaders' contributions are strengthened in a scenario of intergroup competition.[5] Our use of the strategy method when eliciting followers' choices will help to shed light on this issue. In summary, we state the following hypotheses with respect to role-specific behavior:

Hypothesis 5.2. *Leaders contribute more in a social dilemma when it is embedded intergroup competition than when it is not.*

Hypothesis 5.3. *Followers react more cooperatively to the leader's example when the social dilemma is embedded in intergroup competition.*

According to Bornstein and Ben-Yossef (1994), the motivational effect of intergroup conflict might be mediated by group identification. Social identity theory (or SIT), as introduced by Tajfel and Turner (1986), promotes the idea that actions are influenced by the social category of the decision maker. Key to SIT are the assumptions that people strive for a positive self-concept, that their group-membership can provide them with a value which contributes positively or negatively to their self-concept, and that these evaluations come from favorable or unfavorable comparisons with other groups (see Tajfel and Turner, 1986, p. 16). Not every possible comparison matters, however. Only if the decision maker identifies with a group, he will care about the outcome of a comparison. Intergroup competition has the potential to affect personal attachment to a group as

[5] Huck and Rey-Biel (2006) explain the effect of leading by example by way of assuming conformist follower types. In their model, an increased tendency for conditional cooperation would translate to a higher degree of conformism.

it is said to serve "[...] as a unit-forming factor, that enhances group identification [...]" (Bornstein and Ben-Yossef, 1994, p. 64). Research on social identity has also picked up in economics. A recent experiment by Chen and Li (2009) investigates the mechanism underlying the effect of group identification. The results connect to the findings in Halevy et al. (2008). They demonstrate that social preferences may be affected by group identification in that the likelihood for positive reciprocal and social welfare maximizing actions increases. The original result in Bornstein and Ben-Yossef (1994) was replicated several times (Probst et al., 1999, Baron, 2001, Tan and Bolle, 2007). None of these studies directly tested whether the motivational effect of competition works through increased group identification. By measuring identification, our results will shed light on this possible mechanism.

5.3 Experimental Design

5.3.1 The Basic Game

The basic game follows the taxonomy of a standard linear voluntary contribution mechanism (Isaac et al., 1984). Participants interact in groups of $N = 3$ for $t = 1, \ldots, T$ periods. At the beginning of every period, each participant is given an endowment of $E = 10$ ECU (Experimental Currency Units) which she can consume privately or contribute to a group project. Every ECU that is consumed privately benefits the individual decision maker 1 ECU. Every ECU contributed to the group project benefits every member of the group $\beta = 0.5$ ECU. Thus, the payoff for individual i in period t is given by

$$\pi_{i,t} \quad = \quad 25 - c_{i,t} + 0.5 \times \sum_{j \in G_i} c_{j,t} \quad , \tag{5.1}$$

where $c_{i,t}$ is individual i's contribution to the public project in period t, G_i is the set of members in individual i's group, and $\sum_{j \in G_i} c_{j,t}$ are the total (i.e., the sum of) contributions to the public good in individual i's group in that period. Since $\beta < 1$, a pure money maximizer's dominant choice is to contribute nothing to the group project. This result holds for one-shot interactions and can be generalized

to finitely repeated interactions by means of backward induction if we assume rational monetary payoff maximizers and common knowledge. Socially optimal behavior, on the other hand, would prescribe full contribution to the public good, since $N \times \beta > 1$.

5.3.2 Implementing Leading by Example and Intergroup Competition

In the basic game, interaction takes place simultaneously. Leading by example is implemented by means of a semi-sequential decision protocol: one member of the group is randomly appointed to be the "leader" who decides about his contribution to the group project before the other group members do. The leader's decision is communicated to the two other group members, or the "followers," who then decide simultaneously about their contributions.[6] Since, in a last period, contributing nothing is still the dominant choice for followers, the first mover is also always better off contributing zero. I.e., assuming monetary payoff maximizers, the semi-sequential move structure does not alter the behavioral predictions.

Our implementation of intergroup competition follows Bornstein and Ben-Yossef (1994). It involves real payoff consequences but is designed to preserve the intragroup incentive structure of the public goods game.[7] Pairs of groups are formed and after participants decided on their contributions, the groups' total contributions are compared in every pair. This comparison takes place after each period. The group with the higher total contributions wins the comparison and receives a transfer from the loosing group. The transfer equals 0.3 times the absolute difference in total contributions between the opposing groups. Its benefits and costs are shared equally by the respective groups' members. In order to account for the fact that an individual's marginal per capita return (MPCR) of contributing 1 ECU is increased by $\alpha = 0.1$ due to the transfer, we reduce the return from the group project to $\beta_c = \beta - \alpha = 0.4$. In the treatments with intergroup

[6] The instructions used neutral wording: leaders were described as "first movers."

[7] The most significant deviation from the setup in Bornstein and Ben-Yossef (1994) is that the contribution decisions are no longer dichotomous.

competition, individual i's payoff in period t can be summarized by

$$\pi_{i,t} = 25 - c_{i,t} + 0.4 \times \sum_{j \in G_i} c_{j,t} + 0.1 \times \left(\sum_{j \in G_i} c_{j,t} - \sum_{j \in G_{-i}} c_{j,t} \right) \quad , \qquad (5.2)$$

where G_{-i} is the set of members in the group which opposes individual i's. If we assume common knowledge of payoff maximizer preferences and rationality, full free riding remains the theoretical prediction also with intergroup competition, since the MPCR is still below unity.[8] Moreover, we control for the overall MPCR ($\beta = \beta_c + \alpha = 0.5$) such that the marginal incentives to contribute are identical in all conditions. This is necessary since a higher MPCR empirically yields higher contributions (Ledyard, 1995) and would thus constitute a possible confounding effect.[9] Since the transfer constitutes a zero-sum transaction, the overall efficiency for pairs of groups is reduced in treatments with intergroup competition. Preferences for efficiency (see, e.g., Engelmann and Strobel, 2004) would therefore predict contributions to be lower than in the case of isolated groups. This effect runs counter our hypothesis that intergroup conflict promotes intragroup cooperation. Note that, unlike in Bornstein and Ben-Yossef (1994), socially optimal behavior would still prescribe full contributions as $\beta_c \times 3 > 1$. This corresponds to our view that competition can be efficiency-enhancing in an organizational context.

5.3.3 The Experiment's Structure

One-Shot Interaction - Stage 1

The experiment is divided into two stages. The first stage addresses sequential decisions with and without intergroup competition. The two treatments are labeled *S1-SeqC* and *S1-SeqNC*, respectively, and are played between-subjects. Interaction in this stage is one-shot (i.e., $T = 1$) in order to abstract from potential reputation effects (Kreps et al., 1982). In this one period, leaders move first but followers are not informed about their choices. Instead, we employ the strat-

[8] Other ways of implementing competition include awarding fixed exogenous prices (see, e.g., Nalbantian and Schotter, 1997) or Tullock-like contests (see, e.g., Abbink et al., 2010). These approaches do, however, yield Nash equilibria with non-negative contributions.

[9] A control for the overall MPCR is missing, e.g., in Puurtinen and Mappes (2008).

egy method (Selten, 1967) to elicit the followers' conditional responses to each possible contribution by the leader.[10] After collecting contribution choices, we additionally elicit the participants' identification with their group and their perception about the competitiveness of the situation.[11] Participants were informed about the decisions in the first stage only after the end of the second stage to prevent behavioral spill-overs across the two stages and in order to keep the group identification elicitation clean.

Repeated Interaction - Stage 2

The second stage contains a 2×2 between-subjects design with simultaneous vs. sequential decisions as the first dimension and intergroup competition vs. no intergroup competition as the second. The treatments are labeled *S2-SimNC* and *S2-SimC* for the simultaneous move conditions without and with intergroup competition and, accordingly, *S2-SeqNC* and *S2-SeqC* for the sequential move conditions. In all second stage treatments participants interact in a ($T =$) 10 periods repeated partners design with feedback after every period. The feedback contains information about individual contributions in a participant's own group and, in case of competition, the total contributions of the opposing group. We also elicit first order action beliefs about the average contribution in the own group (excluding the leader) and the average contribution in the competing group, where applicable. These beliefs are incentivised following the procedure in Fischbacher and Gächter (2010): if the expectation differs by 0 (1) ECU from the rounded average contribution, the participant receives 3 (2) ECU. In all other cases the participant receives nothing.[12]

[10] It has been argued that employing the strategy method facilitates (cold) decisions based on reason rather than (hot) decisions based on emotions. The empirical evidence on this issue is, however, inconclusive (see, e.g., Brandts and Charness, 2011). If follower behavior is based on emotions and if competition influences those emotions, our use of the strategy method might results in smaller behavioral differences associated with intergroup competition.

[11] See the procedures for the exact wording of the items.

[12] Gächter and Renner (2010) report that this form of belief elicitation can affect contribution decisions. We are, however, mainly interested in treatment differences and it is not obvious why the belief elicitation should affect behavior differentially in different treatments.

Table 5.1: Experimental Treatments

	Stage 1		Stage 2			
Treatment	S1-SeqNC	S1-SeqC	S2-SimNC	S2-SeqNC	S2-SimC	S2-SeqC
Sequential decisions	✓	✓	–	✓	–	✓
Intergroup competition	–	✓	–	–	✓	✓
Strategy method for second mover	✓	✓	–	–	–	–
One-shot	✓	✓	–	–	–	–
10 periods	–	–	✓	✓	✓	✓
Belief elicitation	–	–	✓	✓	✓	✓
# sessions	4	7	2	2	3	4
# groups	25	52	13	12	24	28
# subjects	75	156	39	36	72	84

Note: A checkmark (dash) means that a design feature is (not) present in the respective treatment. The matching across stages is such that participants of treatment S1-SeqNC continue either with treatment S2-SimNC or S2-SeqNC and that those who participated in S1-SeqC continue either with treatment S2-SimC or S2-SeqC.

Matching and Payment

Every subject participates in both stages of the experiment. We keep group composition and the matching into pairs of groups constant across stages.[13] All groups that participate in treatment S1-SeqNC are divided equally to continue either in treatment S2-SimNC or in S2-SeqNC. All pairs of groups that participate in treatment S1-SeqC are divided equally to continue either in treatment S2-SimC or in S2-SeqC. This procedure insures that participants experience either the competition or the no competition environment but not both. Whenever decisions are recorded sequentially in the second stage, the group's leader was also the leader in the first stage of the experiment. This was known to the participants. Table 5.1 summarizes the structure of the experiment.

Participants were paid according to their decisions in only one of the two

[13]Participants learn this not until the beginning of the second stage.

stages. This method prevents the possibility of hedging behavior across stages. Payment for the first stage equals the earnings according to the contribution decisions. Payment for the second stage equals the sum of the earnings from the contribution decision and those for the accuracy of beliefs in one randomly selected period.[14]

5.3.4 Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted in the experimental laboratory of the Max Planck Institute of Economics in Jena, Germany. The participants were undergraduate students from the Friedrich Schiller University Jena. They were recruited using the ORSEE system (Greiner, 2004). Upon arrival, participants were seated at visually separated computer terminals. The instructions were divided into two sets according to the separate stages and distributed before the start of each stage.[15] All instructions were read aloud. The first set announced the experiment's division into two stages but did not specify further information about the second stage. It was common knowledge that only one stage would randomly be selected for payment. Before the start of the first stage, the subjects' understanding was tested by means of a set of control questions. Participants' questions were answered privately at the their seats.

The payoff relevant stage was determined via a coin toss at the end of the experiment. If the second stage was selected, the payoff relevant period was determined by drawing a ball from an urn which contained 10 balls numbered from 1 to 10. Both random draws were performed by one subject and applied to all participants in a session. The subject was chosen based on an experimenter's draw from a second urn. To be as credible as possible, all draws were performed publicly.

As a measure of perceived competitiveness and identification participants were asked to rate their agreement to specific statements on 7 point Likert scales (1="not at all" to 7="very much"). Perceived competitiveness was measured us-

[14] Paying only one randomly chosen period theoretically controls for wealth effects on risk attitudes (Lee, 2008). Paying both the contribution decision and the belief statement in principle allows for hedging risks between these two activities. Blanco et al. (2010) investigate this issue in a sequential prisoner's dilemma game and find no evidence for such hedging behavior.

[15] All sets of instructions can be found in Appendices D.2 and D.3.

ing the statement "I perceived the situation among groups to be very competitive." The items measuring identification were taken from Leach et al. (2008) and read "I feel committed to [In-group]," "I am glad to be [In-group]," "I feel solidarity with [In-group]," and "It is pleasant to be [In-group]." Groups were identified with an individual color.

All sessions were conducted between September and December 2010. The number of sessions, groups, and subjects per treatment are summarized in Table 5.1. Overall, 231 subjects participated in the experiment. Sessions lasted between one and a half and two hours. Given the exchange rate of 0.80 euro cents per ECU, subjects earned on average 12.70 euro, ranging from 6.50 euro to 21.80 euro (inclusive of a 2.50 euro show-up fee).

5.4 Results

Results are going to be presented as follows. First we provide a manipulation check. Next, we report on differences in leader and follower behavior with and without intergroup competition using first stage data. Afterward, we focus on the results for the repeated interactions in the second stage.

5.4.1 Manipulation Check

We implemented intergroup competition via a transfer between groups. In order to check whether the manipulation was successful, we compare the perceived competitiveness across conditions in stage 1. Since the first stage does not include any feedback, every participant's response can be treated as an independent observation. With 75 and 156 observations and average measures of 2.8 and 4.2 for S1-SeqNC and S1-SeqC, respectively, the difference is highly significant ($p < 0.01$, Wilcoxon rank sum test, two-sided). We are thus confident that we successfully manipulated the perception about the environment. Next, we test the conjecture that competition yields higher identification with the in-group. We calculate each individual's mean identity score as the average response to all four items. Comparing conditions S1-SeqNC and S1-SeqC, the mean identity scores

are virtually identical at 3.65 ($p = 0.82$, Wilcoxon rank sum test, two-sided).[16] This provides an indication that intergroup competition might affect behavior in other ways than by inducing in-group identification. On the individual level the measures of identification and competitiveness are correlated in S1-SeqC (Spearman's $\rho = 0.35$, $p < 0.01$) but not in S1-SeqNC ($\rho = 0.12$, $p = 0.29$). Over the full sample, the Spearman correlation coefficient is $\rho = 0.26$. While this correlation is significant ($p < 0.01$), its magnitude points to a rather weak interdependence of perceived competitiveness and group identification.

5.4.2 One-Shot Interaction

Overall contributions. We begin the analysis examining average group behavior. The first stage involves a pure one-shot interaction. However, since followers' decisions are conditional on those of leaders, each group constitutes the basis for one independent observation. This leaves us with 25 and 52 independent observations for S1-SeqNC and S1-SeqC, respectively. A group's average contribution is calculated as the mean of the leader's contribution and the two followers' *actual* contributions. The latter are those conditional choices of followers which correspond to the leader's actual decision. Following our first hypothesis, we expect higher contributions with intergroup competition. The mean contributions are depicted in the first two bars in Figure 5.1, panel a. They amount to 3.84 ECU in S1-SeqNC and 4.53 ECU in S1-SeqC. The difference is significant ($p = 0.049$, Wilcoxon rank sum test, one-sided), which confirms our expectation. This yields our first result:

Result 5.1. *Three person groups consisting of one leader (first mover) and two followers (second movers) contribute more to a public good when the social dilemma is nested in an intergroup conflict.*

Leader and follower behavior. To what extend can this effect be attributed to differences in leader and follower behavior with and without intergroup competition? Bars three to six in panel a of Figure 5.1 depict the role-specific contributions for both experimental conditions. Hypothesis 5.2 postulates higher contributions from leaders in the presence of intergroup competition. The figure, however,

[16] There are no significant differences for responses to any of the single items.

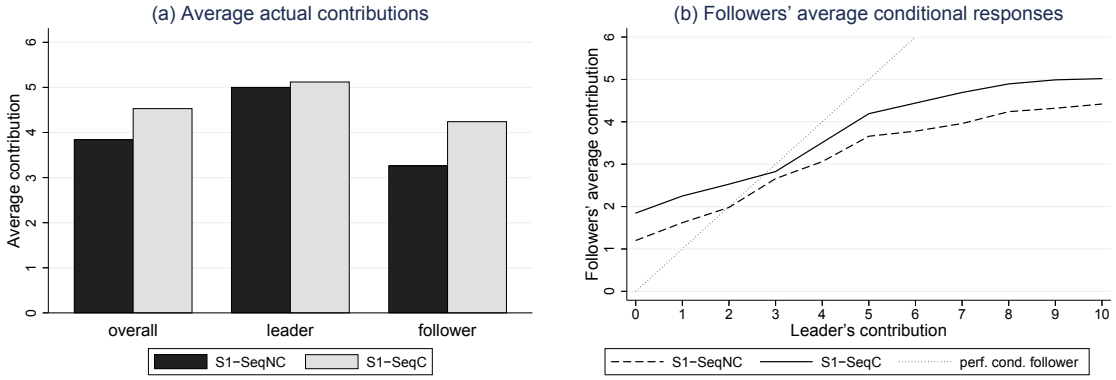


Figure 5.1: Average Contributions in Stage 1

suggest that leaders' choices remain unaffected by competition This impression is corroborated by a one-sided Wilcoxon rank sum test ($p = 0.35$). How do followers react? Visual inspection suggests that followers' actual contributions are higher in S1-SeqC than in S1-SeqNC. We use a Wilcoxon rank sum test for statistical analysis. Since, in each group, both followers' actual contributions depend on the same leader decision, we calculate their average as one independent observation. Using the resulting 25 and 52 observations for S1-SeqNC and S1-SeqC, respectively, the test confirms that intergroup competition leads to higher follower contributions ($p = 0.023$, Wilcoxon rank sum test, one-sided). In connection with the observation that leaders' contributions are seemingly insensitive to intergroup conflict, this finding supports Hypothesis 5.3: a leader's example is followed more closely if the social dilemma is embedded in intergroup competition.

Followers' conditional responses. The differences in followers' actual contributions could be driven by heterogeneity in leaders' choices which is not captured by their average. The followers' full conditional choice vectors control for this issue. Figure 5.1, panel b, displays the average vectors by experimental condition.[17] The monotonically upward sloping lines clearly show that followers on average react positively to an increase in the leader's contribution. Nonetheless, the most critical result from previous studies on leading by example proves to be robust: followers tend to undercut the leaders' contributions. This can be seen

[17]The average conditional choice vectors are obtained by calculating the followers' mean response for each possible leader contribution.

when comparing the average vectors to that of a hypothetical, perfect conditional follower depicted by the dotted line. It is only for small (<3 ECU) leader contributions that followers contribute the same as or more than leaders. The figure suggests furthermore that followers sustain systematically higher contributions under intergroup competition. We first investigate this issue by calculating every follower's average conditional response. The averages of this measure amount to 3.17 ECU in S1-SeqNC and 3.75 ECU in S1-SeqC, where the figures are based on 50 and 104 independent observations, respectively (one for each follower). The difference is small but statistically significant ($p = 0.039$, Wilcoxon rank sum test, one-sided). This finding indicates that followers' average responses are higher under intergroup competition. Next, we perform an individual regression on each follower's vector of conditional choices and compare the resulting slopes across conditions. This measure can be used to investigate the followers' average marginal responsiveness as higher values indicate that the followers react more strongly to changes in the leaders' decisions. The mean slopes amount to 0.34 and 0.35 in S1-SeqNC and S1-SeqC, which confirms that followers on average react positively but far from perfectly to a change in the leader's contribution. A Wilcoxon rank sum test fails to reject the null hypothesis that the mean slopes come from the same underlying distribution ($p = 0.78$, two-sided). The difference in followers' average contributions is thus not reflected by steeper reaction functions with respect to the leader's examples. On average, followers rather seems to increment their contribution by an amount which is not conditional on what the leader does. Evidence in support of this conjecture comes from a Wilcoxon rank sum test that compares the followers' predicted responses for the average contribution of a leader (i.e., 5 ECU). We use this measure as a proxy for the constant part of the followers' response functions. The test rejects the null hypothesis at the 10% significance level ($p = 0.08$, two-sided). The evidence on role-specific behavior is summarized by:

Result 5.2. *The presence of intergroup competition does not affect the contribution behavior of leaders but the conditional responses of followers. The change in follower behavior is best described as an increase in the willingness to contribute which is not conditional on the leader's decision.*

There is considerable heterogeneity in how followers react to leaders' contributions. As this study is the first to use the strategy method on followers' choices, we are able to provide a more complete picture of these patterns. We categorize followers into five distinct groups according to the pattern of their conditional responses. The first group are *strict non-contributors (SNC)* who contribute exactly zero ECU for every possible leader decision.[18] *Unconditional contributors (UC)* do also not condition on the leader's decision but contribute strictly positive amounts. *Conditional followers (CF)* are characterized by upward sloping conditional response vectors. Their vectors of conditional choices either increase monotonically with the leader's contribution or exhibit a positive and highly significant Spearman correlation coefficient.[19] The fourth group are *hump-shaped followers (HSF)* who react positively to better examples only up to some specific leader contribution. Beyond this threshold, they react negatively.[20] The last group are *reverse conditional followers (RCF)* who contribute less the more the leader contributes.[21] Such a pattern might reflect a motivation to supply some fixed amount of the public good as a group. A higher leader contribution would lower the burden to contribute for the follower, which implies a downward sloping pattern. The group *no category (NC)* subsumes all remaining followers. Table 5.2 depicts the observed relative frequencies for each follower category. It is evident that conditional followers constitute the largest group. Strict non-contributors, reverse conditional followers, and hump-shaped followers mark the second, third, and forth most frequently observed categories. Unconditional contributors are rarely observed. Note that about one fourth of all followers does not fall into any category. This is partly due to our strict requirement on the significance of the Spearman correlation coefficient.[22] Comparing the distributions of types across treatments, we

[18] We prefer this term to *free riders* as the latter is already established and describes an actor who contributes nothing independently of the decisions of everyone else in the group (see, e.g. Fischbacher et al., 2001).

[19] These criteria mirror those used in Fischbacher et al. (2001) and Fischbacher and Gächter (2010) for a public goods game with simultaneous decisions.

[20] The decisive criterion is a highly significant positive Spearman correlation coefficient for choices smaller or equal to the threshold and a negative and highly significant coefficient for choice above the threshold.

[21] The conditional responses either monotonically decrease with higher leader contributions or exhibit a negative and highly significant Spearman rank correlation coefficient.

[22] A relaxation of this requirement yields more observations for conditional followers, reverse conditional followers, and hump-shaped followers.

Table 5.2: Relative Frequencies of Follower Categories

Treatment	SNC	UC	CF	RCF	HSF	NC	# obs
Isolated groups (S1-SeqNC)	10.0	6.0	46.0	8.0	6.0	24.0	50
Competition (S1-SeqC)	10.6	2.9	40.4	8.7	6.7	30.7	104
Total	10.4	3.9	42.2	8.5	6.5	28.5	154

Note: Abbreviations: SNC = strict non-contributors; UC = unconditional contributors; CF = conditional followers; HSF = hump-shaped followers; RCF = reverse conditional followers; NC = no category.

find them to be very similar in both conditions. A one-sided Fisher exact test does not reveal a significant difference in the distribution of types ($p = 0.89$). Pairwise comparisons for each individual category also do not indicate any differences in the relative frequencies (the smallest p-value is equal to 0.25 for *no category*, one-sided). Intergroup competition thus does not seem to have an influence on the distribution follower types. Figure 5.2 displays the average contribution vectors by follower category for both experimental conditions. It clearly shows the distinct type-specific patterns of conditional responses. The figure indicates furthermore that the increase in followers' average conditional responses which is associated with intergroup competition (see Result 5.2) is mainly due to unconditional contributors, reverse conditional contributors, and those who cannot be categorized.[23] In contrast, conditional followers display almost identical average conditional response vectors in both experimental conditions. We summarize as follows:

Result 5.3. *Followers can be classified into several types according to their conditional responses to leaders' examples. While almost half are conditional followers, we also observe strict non-contributors, unconditional contributors, reverse conditional followers, and hump-shaped followers who each account for a minor percentage of the observations. The distribution of types does not depend on the presence of intergroup competition.*

[23] Wilcoxon rank sum tests that compare followers' average conditional responses between S1-SeqNC and S1-SeqC, however, do not reveal statistically significant differences for any single follower type.

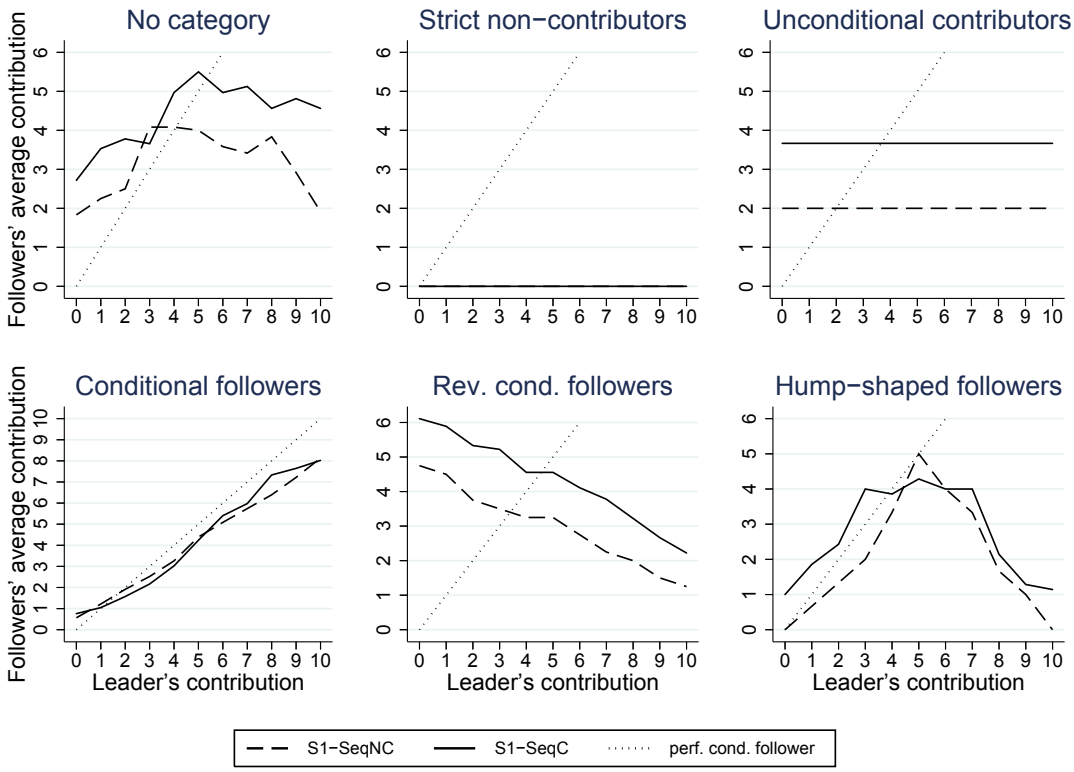


Figure 5.2: Followers' Average Conditional Responses by Follower Category

5.4.3 Repeated Interaction

Investigating one-shot interactions allows for valuable insights as it abstracts from strategic behavior. Real life social dilemmas such as team work situations, however, are often marked by repeated interaction which allows for reputation building or reciprocity concerns. In this section, we extend our analysis to repeated play using data from the second stage of the experiment. Most of the analysis in this section is based on independent observations. Due to the feedback between periods, the unit of an independent observation are group averages in case of isolated groups and averages of pairs of groups in case of intergroup competition.

Treatment effects. Figure 5.3 graphs the time series of average contributions for each treatment. Treatment S2-SimNC replicates standard findings closely as contributions start at around 50% of the endowment and decline steadily to 20%

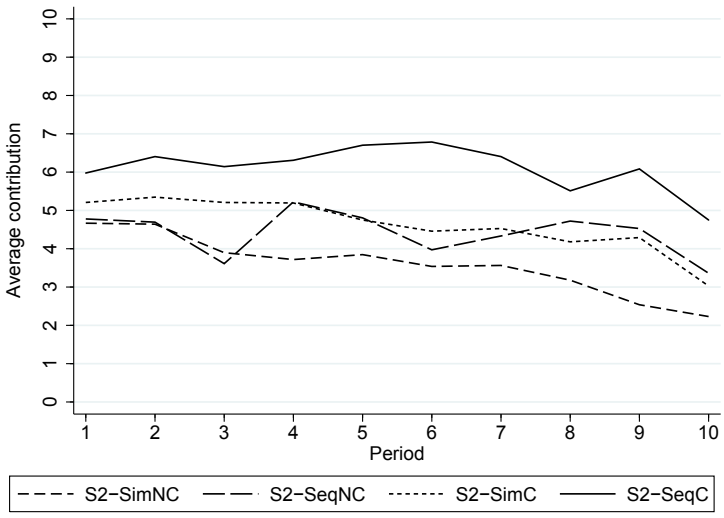


Figure 5.3: Average Contributions Over Time in Stage 2

of the endowment in the last period. Contributions in S2-SimC are higher than without competition in every period but show the same decline over time. Both treatments with sequential decisions show higher cooperation rates than the respective simultaneous move conditions. While this improvement is only visible for periods 4 to 10 when comparing S2-SeqNC to S2-SimNC, it is sustained over all ten periods for the treatments with intergroup competition.

In order to assess how the treatments affect overall cooperation, we average contribution over all ten periods. Table 5.3 provides the relevant descriptive statistics, based on independent observations. Mean and median contributions indicate what Figure 5.3 already suggested: intergroup competition seems to foster intragroup cooperation. Pairwise two-sided Wilcoxon rank sum tests, however, fail to reject the null hypothesis that contributions come from the same underlying distribution both for simultaneous ($p = 0.18$) and for sequential decisions ($p = 0.14$). Thus, while intergroup competition leads to higher contributions in our (sequential move) one-shot interactions, this does not prove to be robust in repeated play.[24] In order to assess whether we find the effect of intergroup competition in our overall data set, we pool the data for the simultaneous and

[24]This result is in accordance with previous findings. While Bornstein and Ben-Yossef (1994) and Baron (2001) show that cooperation is higher in the IPD than in the PD for one-shot interactions, this relation is not confirmed in Bornstein et al. (1996) who allow for repetition.

Table 5.3: Descriptive Statistics: Time-Averaged Contributions Based on Independent Observations

Treatment	Mean	Median	St. Dev.	# Indep. obs.
S2-SimNC	3.6	3.4	1.96	13
S2-SeqNC	4.4	5.1	2.83	12
S2-SimC	4.6	4.5	1.71	12
S2-SeqC	6.1	6.5	1.59	14

sequential treatments. A comparison based on independent observations shows that the difference is statistically significant ($p = 0.036$, Wilcoxon rank sum test, two-sided).

Next, we consider leading by example. Figure 5.3 and Table 5.3 indicate that sequential decisions tend to elicit higher average contributions both with and without intergroup competition. We test this formally using two-sided Wilcoxon rank sum tests. While the test widely fails to reject the null hypothesis that contributions stem from the same underlying distribution when there is no intergroup competition ($p = 0.39$), the null hypothesis is rejected for the condition with intergroup competition ($p = 0.045$). Thus, according to our data, leading by example only has a consistent positive effect on contributions when it is combined with intergroup competition. This also drives the significant effect of sequential vs. simultaneous decisions when the data is pooled over conditions ($p = 0.037$, Wilcoxon rank sum test, two-sided). The non-significance of the effect of leading by example without intergroup competition runs counter to results in previous studies. It is, however, not possible to attribute this discrepancy to a specific element of the experimental design since neither of the previous studies on leading by example used three person groups, payed only one period in an repeated partners design, or elicited beliefs. The relative increase in contributions of 22% is, moreover, in the same order of magnitude as the ones found in previous studies.[25] We summarize as follows:

Result 5.4. *Both, leading by example and intergroup competition, significantly increase contributions to the public good. The effect of leading by example is only significant in the condition with intergroup competition.*

[25]Güth et al. (2007), for instance, find a weakly significant increase of 33%.

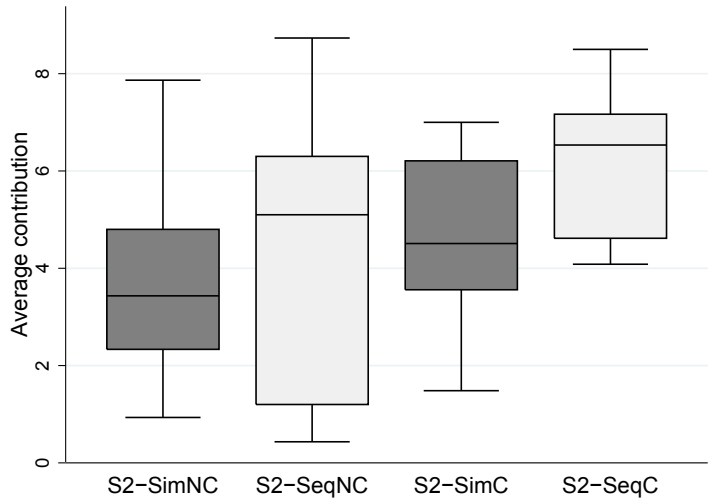


Figure 5.4: Box-Whisker Plots of Time-Averaged Contributions Based on Independent Observation. The Boxes Depict the 25 Percentile, the Median, and the 75 Percentile. The Whiskers Mark the Upper and Lower Adjacent Values.

A noteworthy feature of the data is that the variation in cooperation between matching-groups depends on the treatment. Figure 5.4 provides a graphical illustration. The Box-Whisker plots indicate that the variation between matching-groups is largest in S2-SeqNC and smallest in S2-SeqC. In fact, S2-SeqNC shows the highest maximum and the lowest minimum average contributions among all treatments. Comparing the standard deviations of time-average contributions based on independent observations between S2-SeqNC and S2-SeqC, this difference is significant ($p = 0.026$, robust Levene's-test).[26] It thus seems that intergroup competition does not only induce significantly higher average contributions when decisions are sequential but also a reduction in variance. The large variance in S2-SeqNC also explains why leading by example does not seem to have an effect without competition. It is only with intergroup competition that sequential decisions lead to consistently higher contributions. This result complements the finding in Sausgruber (2009) who shows that intergroup comparison reduces between-group variance in cooperation.

[26]None of the other pairwise comparisons reveals significant differences for the respective standard deviations.

Interaction of leading by example and intergroup competition. The presence of the leader yields a relative increase in contributions which amounts to 22% without competition and to 39% with competition. The absolute increase, although being of a small order of magnitude, roughly doubles. This suggests that leading by example and intergroup competition may interact in their effects on overall contributions. In order to formally test for such an interaction effect, we turn to regression analysis. Table 5.4 presents results from two panel Tobit models, with subject-specific random intercepts.[27] The first model regresses individual contributions on the two dummy variables *Competition* and *Sequential* representing the experimental conditions, a linear trend, *Period*, as well as on *Age* and *Gender* (male=1). The results partly mirror those obtained from the non-parametric tests since both main effects turn out to be positive and significant. In addition, we obtain the expected negative trend over periods and a positive effect of age. The second model augments the first as it adds the interaction of both experimental conditions labeled as *Comp*Sequential*. As expected, its point estimate is positive which indicates that the effect of leading by example tends to be larger with intergroup competition. However, the interaction is not significantly different from zero. The inclusion of the interaction also does not improve the model fit ($p = 0.19$, LR-test). It is thus not possible to conclude that leading by example and intergroup competition interact in a meaningful way in their effects on overall cooperation.

Our estimations do not allow contribution decisions to be interdependent within the same group and period. This, however, is likely to be the case as subjects condition their behavior on the history of choices and because of the semi-sequential decision protocol in the treatments with leading by example. In order to control for this issue, we estimate two Tobit models with group-specific random intercepts and two linear mixed effects models with nested random intercepts for groups and individuals.[28] All previous results are shown to be robust (see Tables D.1 and D.2 in Appendix D.1).[29] We summarize as follows:

[27] We report on Tobit models since the dependent variable (individual contributions) exhibits a large number of corner solution outcomes. In fact, 32% of all observations are either 0 ECU or 10 ECU.

[28] Due to a lack of implementation in Stata 11, we are not able to estimate Tobit models with nested random intercepts for groups and individuals.

[29] The only difference is that the coefficient for *Competition* turns out to be significant in the linear

Table 5.4: Panel Tobit Regressions: Main and Interaction Effects

Dep. variable: individual contributions Random intercept: individual				
	(1)		**(2)**	
	Coefficient	95% CI (BCa)	Coefficient	95% CI (BCa)
Competition	1.896**	[0.32; 3.22]	1.213	[-0.50; 2.94]
Sequential	1.629***	[0.44; 2.76]	0.703	[-1.91; 2.99]
Comp*Sequential	–	–	1.356	[-1.17; 4.64]
Period	-0.212***	[-0.34; -0.11]	-0.212***	[-0.34; -0.11]
Age	-0.231***	[-0.41; -0.09]	-0.224***	[-0.39; -0.09]
Gender	-0.340	[-1.16; 0.28]	-0.268	[-1.10; 0.46]
Constant	9.044***	[5.33; 12.51]	9.307***	[5.90; 13.26]
St. dev. random intercept	3.470***	[2.90; 3.98]	3.458***	[2.90; 3.99]
St. dev. residual	3.044***	[2.73; 3.65]	3.044***	[2.74; 3.65]
Log likelihood	-4838.2		-4837.3	

Note: The regressions are based on 2310 observations, 231 individuals, and 77 groups. 448 (293) observations are left (right) censored. The bias corrected and accelerated (BCa) confidence intervals are based on non-parametric bootstraps with 500 replications. Sampling respects group composition. ***, **, * indicate significance at levels 1%, 5%, and 10%, based on the BCa confidence intervals.

Result 5.5. *The interaction effect of leading by example and intergroup competition is positive but not statistically significant.*

Leader and follower behavior. Figure 5.5 depicts the time series of leader and follower contribution decisions for isolated groups (panel a) and intergroup competition (panel b). Both graphs show clearly that second movers tend to follow the leaders' examples over time. The Spearman correlation coefficients between leader and average follower contributions are $\rho = 0.77$ and $\rho = 0.68$ (both $p < 0.01$) for treatments S2-SeqNC and S2-SeqC, respectively.[30] At the same time, it is obvious that leaders consistently contribute more than followers. This is corroborated by two-sided Wilcoxon signed rank tests comparing leader and average follower contributions, averaged over periods ($p = 0.007$ in S2-SeqNC and $p < 0.01$ in S2-SeqC). As a consequence, leaders' expected earnings based on contribution decisions are lower than those of followers ($p < 0.01$ in S2-SeqNC and

mixed effects model that includes the interaction effect *Comp*Sequential*.

[30] We average the contributions of the two followers in one group in order to obtain one number which can be compared to the leader's example.

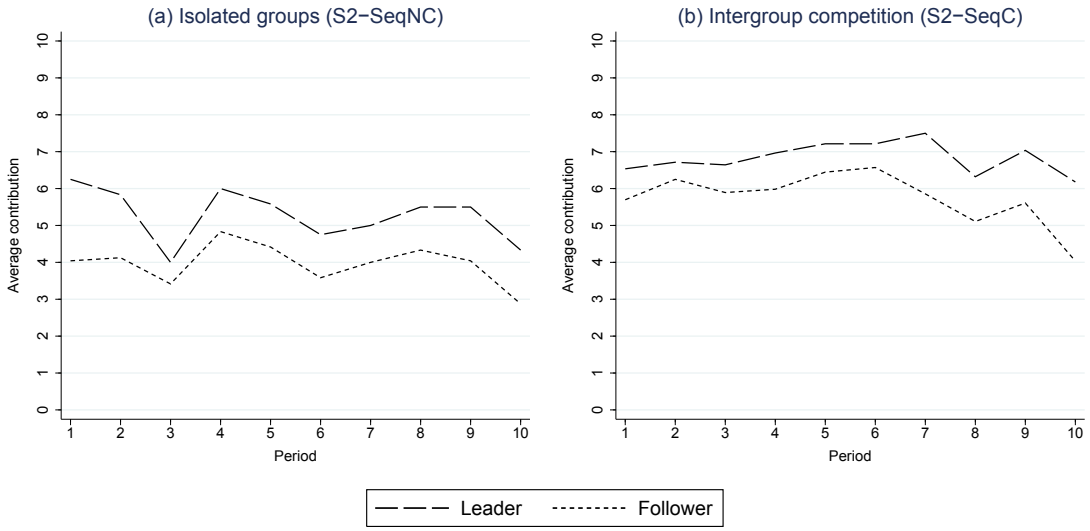


Figure 5.5: Leaders' and Followers' Contributions Over Time for the Conditions Without (Panel a) and With Intergroup Competition (Panel b)

$p < 0.01$ in S2-SeqC, two-sided Wilcoxon signed rank test).[31] These results are in accordance with those from previous studies (see, e.g., Levati et al., 2007).

5.5 Conclusions

In this study we experimentally investigate leading by example in a linear public goods game in environments with and without intergroup competition. The advancement with respect to the previous literature is two-fold.

First, we use the strategy method to characterize followers according to their conditional responses to a leader's contributions. Our results suggest that the usually observed undercutting of the leaders' examples may be the result of type-specific behavior. While the largest group of participants are conditional followers who reciprocate a leader's example, other types like strict non-contributors or hump-shaped followers on average undercut the leader's contribution. A non-negligible fraction of followers even punishes better examples by means of re-

[31] Note that payment for the second part was based on one randomly chosen period and incorporated payment for belief statements. The tests compare expected earnings for leaders and followers (averaged per group) based on contributions, i.e., excluding those from belief statements.

verse conditional behavior.

Second, we generalize the cooperation enhancing effect of leading by example to a scenario of intergroup competition. Moreover we show that intergroup competition has differentiated effects on leading by example. While leader behavior remains largely unaffected, followers behave more cooperatively in the presences of intergroup competition. Their change behavior is best described as an increase in the willingness to cooperate that is not conditional on the leader's contribution. Thus, while exemplary effort should always be encouraged, it seems even more beneficial when groups are in competition. The latter statement, however, hinges on our specific parametrization in that a cooperative act benefits the in-group more than it hurts the out-group. The effects of leading by example in destructive intergroup conflict remain to be explored.

In this study, we control for the incentive effects of intergroup competition in order to examine its purely motivational aspects. In real life, however, both these effects are present simultaneously. A possibly fruitful avenue of research is thus to investigate the interplay between leading by example and the structural effects of intergroup conflict. A leader's example might, e.g., constitute a powerful tool for equilibrium selection in contests that are associated with an exogenously given price (see, e.g., Erev et al., 1993, Abbink et al., 2010) or all-can-win competitions (Reuben and Tyran, 2010).

CHAPTER 6

SUMMARY AND CONCLUDING REMARKS

This thesis addresses the question how to promote voluntary cooperation in social dilemmas. In what follows, we summarize the main findings of the four studies presented in this thesis and provide some concluding remarks.

Chapter 2 starts out from the current state of the literature which holds that mutual promises are crucial for the cooperation-enhancing effect of (multi-way) communication (see, e.g., Balliet, 2010). The predominance of mutual promises is also consistent with a broader view that builds on coordination among conditional cooperators to account for the effects of communication (see, e.g., Fehr and Fischbacher, 2002). Since promises are only one of many conceivable coordination devices, their necessity is questionable. We investigate this issue in a series of public goods experiments that only allow for one-way communication, i.e., a free-form written message by a single member of a group prior to the simultaneous contribution decisions. Since all other group members are unable to respond, this mechanism precludes the mutual exchange of promises. Our experimental data show that one-way communication increases cooperation compared to no-communication baseline treatments in pure one-shot as well as in finitely repeated interaction. In the latter, one-way communication is equally effective if a message can be sent only prior to the first or before every period. The communication content analysis indicates that suggestions to contribute efficiently (i.e., the entire endowment) are ubiquitous while unilateral promises to cooperate are infrequent. Overall, the effectiveness of one-way messages demonstrates that mutual promises are not necessary for communication to increase cooperation. The prevalence of contribution suggestions, however, is consistent with the conjecture that communication is used as an opportunity to coordinate behavior.

In Chapter 3, we examine one-way communication in a public goods game with asymmetric experimental endowments. This form of asymmetry has been shown to reduce cooperation (Cherry et al., 2005) and induces behavior consistent with an equal contribution rule (Buckley and Croson, 2006). Such behavior may restrict the potential effect of one-way communication, since the efficient provision requires unequal contributions to the public good when endowments are asymmetric. Our results confirm that "poor" and "rich" subjects contribute similar amounts if communication opportunities are unavailable. One-way communication, however, not only enhances cooperation but also induces behavior consistent with an equal payoffs rule that prescribes efficient contributions.

In Chapter 4, we show that (i) one-way messages which are not related to the experimental game do not significantly affect behavior and that (ii) one-way communication increases contributions to the public good even if cooperation leads to unequal earnings. The first observation indicates that the effectiveness of one-way communication does not merely originate from the socializing function of the messages but rather from their (game-relevant) content. The second observation stems from an experiment in which we compare behavior in groups with symmetric and asymmetric marginal benefits from cooperation. In two no-communication baseline conditions, we show that this form of asymmetry may be hurtful to cooperation since contribution behavior is inversely related to the marginal benefits of other actors. One-way messages counteract this tendency as they increase contributions particularly by those individuals who benefit the least from cooperation. This observation is particularly striking since the messages originate from the main beneficiaries of cooperation and are thus suspected to serve egoistic purposes.

Previous research shows that leading by example, i.e., the exemplary effort of a first mover, promotes voluntary cooperation in public goods games. Its effect on behavior, however, is fragile since followers tend to undercut the leaders' efforts, thereby causing cooperation to dwindle over time. The study presented in Chapter 5 provides a more detailed picture of this phenomenon as it shows that follower behavior is type-specific. While almost half of all second movers are conditional followers who reciprocate a leader's contribution, other types are insensitive to leading by example or even punish exemplary efforts by means of

reverse conditional behavior. This observation might be interesting to group organizers as it indicates that group composition is important for the workings of leadership. A finding that connects this study further to an organizational context is that leading by example affects intragroup cooperation also when groups are in competition. In fact, intergroup competition may even enhance the impact of good examples since it promotes cooperative behavior by followers in particular.

In the studies presented in this thesis, the implementation of one-way communication was predominantly motivated by its usefulness to investigate the mechanisms that underly effective communication. But one-way communication is more than a purposeful experimental manipulation. It is also an experimental adaption of real-world situation in which communication technologies are uni- rather than multidirectional. Recommended contributions in fundraising for charities (Croson and Marks, 2001) or in political campaigning in the U.S. are such instances. In an organizational context, one-way communication can be observed in the form of announcements or speeches by executives. At large events such as demonstrations or pre-election parties, leading activists or politicians also address all participants simultaneously and unilaterally.[1] One-way communication may even occur among actors who meet at eye level in most other situations. Imagine, for instance, a department leader who is a co-worker in one context but the superior in another.

Our implementation of one-way communication is, of course, a lousy simulation of the situations portrayed above and by no means directly applicable. It fails to capture potentially important aspects, only some of which might be accounted for in future laboratory experiments. For instance, while the possibility to adjust for group size is limited, formal authority may be implemented in the form of exclusion power (see, e.g., Güth et al., 2007). Whether such competencies are conducive to the effectiveness of one-way communication or even have crowding out effects is a topic for future research.

The predominance of mutual promises in communication experiments has led to discussions about which communication technologies are more suitable to

[1] The traditional New Year's speech of the German Prime Minister (or Chancellor) is an extreme example of one-way communication.

promote cooperation. Reviewing the literature, Bicchieri and Lev-On (2007) argue that "thin" communication channels like chat or audio conferences are less practical since they impede the efficient exchange and the credibility of promises. The fragility of the communication effect based on promises may cause scepticism with regard to its applicability to situations outside the laboratory. To rely on the effectiveness of promises is especially problematic when its complex requirements may cause high organizational costs (see, e.g., Messick and Brewer, 1983). Consequently, one-way communication may be favorable, where its simplicity is advantageous. Large groups may be such an instance since the number of required links among group members grows more rapidly for multi-way than for one-way communication as group size increases.[2] Noisy communication environments may also favor one-way communication since the informational requirements for effective one-way messages seem less demanding than those for credible mutual promises. By now, no research has been conducted on the relative advantages of one-way and multi-way communication in social dilemmas. Such an endeavor seems promising, however, especially as it may allow for insights with regard to the efficient application of communication outside the laboratory.

Any efficient application of one-way communication necessitates an accurate understanding of the underlying mechanisms. While the experimental results presented in this thesis give some indications, they are not conclusive. The ineffectiveness of irrelevant one-way communication indicates that messages have to relate to the decision situation. The communication content analysis signifies the predominance of suggestions to contribute the entire endowment in conjunction with different supplementary arguments. The behavioral data show that contribution decisions are largely congruent with such suggestions even if experimental endowments or evaluations for the group project are asymmetric. This correspondence of behavior and communication content, however, does not imply that suggestions to contribute efficiently are sufficient or necessary for the effect of one-way communication. In fact, the literature entails some indication that mere suggestions do not suffice to enhance cooperation. In step-level public

[2]This conjecture may also explain why communication among all the members of a group is often encouraged in small groups but very rarely observed in large groups.

goods games, for instance, suggestions to contribute according to the symmetric Nash equilibrium do not affect behavior (Croson and Marks, 2001). In linear public goods games, numerical (multi-way) communication remains ineffective – no matter whether it is framed as contribution announcements (Wilson and Sell, 1997, Bochet et al., 2006) or as promises (Bochet and Putterman, 2009). An unpublished pilot study of our own also indicates that mere (one-way) suggestions may not be sufficient to enhance cooperation.[3] In order to assess the role of suggestions and their interplay with seemingly auxiliary statements, more data is needed. A standardization of the communication protocol might be one way to generate correlated systematic variation in the communication content and the decision behavior. Such an investigation could be arranged based on the communication content analysis of previous experiments and may yield important insights with respect to the underlying mechanism of effective one-way communication.

All four studies presented in this thesis demonstrate the effectiveness of leadership as a tool to alleviate the free-rider problem in social dilemmas. They show that leadership – in the form of, either, leading by words or leading by example – promotes voluntary cooperation in situations as diverse as one-shot and repeated interaction, in isolated groups and intergroup competition, and in situations when efficiency necessitates unequal individual efforts or when cooperation leads to inequality in earnings. Leadership is important in situations that require civil courage, in charitable giving, in the sphere of politics, and in most organizations. This being said, the scarcity of theoretical and empirical economic research on leadership in collective action problems seems unsatisfactory. This thesis provides a small contribution to fill this gap but, in light of the significance of leadership and that of collective action problems, it seems a small contribution to an important topic.

[3]More specifically, we contrasted treatment NC (see Chapter 2) with a treatment that allowed for messages of the following form prior to every period: "I suggest to contribute X ECU to the project." Communicators were allowed to enter their contribution suggestion for X (or to send no message at all). The development of average group contributions over time was almost identical in both treatments.

APPENDIX A

APPENDIX TO CHAPTER 2

A.1 Categorization Methodology

Our categorization methodology follows Cooper and Kagel (2005) and Sutter and Strassmair (2009). Initially, two researchers examined independently a sample of the messages and established their own distinct sets of preliminary categories.[1] Each category represents one or more arguments that the communicator is likely to invoke, and each message may belong to more than one category. After consultations, the two researchers agreed upon the final set of categories shown in Table 2.3.

Then three undergraduate research assistants coded (once again separately) the total of the messages one by one: if one message contained the argument(s) specified by some category, then that category was assigned the value of 1 (otherwise, it was assigned the value of 0). The average correlation coefficient between the assistants' codings ranged from 0.70 to 0.80 (0.81 to 0.89) for the first-period messages sent in CC (PC), and from 0.75 to 0.79 for the messages sent in periods 2–10 in CC.[2] We can not compute such correlations coefficients in the case of C, as the assistants often coded with the same value all the available messages. As an alternative we report that the number of times all coders agreed on 1 relative to the number of times that at least one of them decided on 1 equals 0.85.[3]

Finally, the coders gathered, discussed their individual assessments and arrived at a common coding (the results are reported in Tables 2.4 and 2.5 for the repeated and one-shot treatments, respectively).

[1] Since the experiment was conducted in German, the categorization was undertaken by Johannes Weisser and Matthias Uhl, a German native speaker familiar with all details of the experiment.

[2] The correlation coefficient values reported by Cooper and Kagel (2005) and Sutter and Strassmair (2009) are somewhat smaller, implying that our categorization procedure was more clear-cut.

[3] The corresponding ratios for the PC, CC (period 1), and CC (periods 2–10) treatments are 0.79, 0.83, and 0.61, respectively.

A.2 Instructions for Repeated Interaction Treatments

This appendix reports the instructions (originally in German) for treatments B_{10}, PC, and CC. Those for treatment B_{10} are displayed below in full length. They contain all parts which are common to all three treatments. The instructions for the other treatments can be obtained by inserting and replacing the appropriate paragraphs. The place holder *[for <treatment name>, replace the following paragraph]* indicates which paragraphs have to be replaced, where the replacement always has the same heading. The place holder *[for <treatment name>, insert paragraph <paragraph name> here]* prescribes where new paragraphs have to be inserted.

A.2.1 Instructions for Treatment $\mathrm{B_{10}}$

INSTRUCTIONS

Welcome and thank you for participating in this experiment. Please remain silent and switch off your mobile!

You will receive 2.50 euro for showing up on time. Beyond this you can earn more money. In order to do this, please read these instructions carefully. The 2.50 euro show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., without the other participants knowing the extent of your earnings.

During the experiment, we shall not speak of euros but of ECUs (Experimental Currency Units). ECUs are converted to euros at the following exchange rate: 1 ECU = 0.05 euro.

It is strictly forbidden to speak to other participants. If you have any questions during the experiment please raise your hand.

Detailed information on the experiment

Group formation

You will be placed in a group of four players. Group composition does *not* change during the experiment, i.e., you will be always interacting with the same participants. You will never learn the identity of the other members of your group.

Decisions

The experiment consists of 10 periods. At the beginning of each period, you (as well as the other members of your group) receive an endowment of 25 ECUs. You have to decide **how many of these 25 ECUs you want to contribute to a project**. The ECUs contributed to the project yield income for you as well as for the other members of your group (you will learn more about the "income from the project" below). You can keep the ECUs that you do not contribute for yourself (they yield income just for you).

Period-earnings

More specifically, in every period your earnings consist of two parts:

a) "Income from the project" = 0.4 $\times$ sum of all group members' contributions (in words, the income from the project is determined by multiplying the sum of the contributions of all group members by 0.4);

b) "ECUs you keep" = 25 $-$ your contribution to the project.

Thus, your period-earnings summarized in a formula are

Your period-earnings $=$	**Income from the project**	$+$	**ECU you keep**
	$(0.4 \times$ sum of group's contributions$)$	$+$	$(25 -$ your contribution$)$

Example:

Suppose that all four group members contribute 5 ECUs. Then both you and your group members receive an "income from the project" of 8 (= 0.4 $\times$ 20) ECUs. The "ECUs you keep" are 20 ($= 25 - 5$). Hence, your period-earnings are 8 + 20 = 28 ECUs.

[*for treatments PC and CC, replace the following paragraph]*

Interaction with your group members in each period

In each period, you as well as the other three members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

[*for treatments PC and CC, insert paragraph Communication here]*

The information you receive at the end of each period

At the end of each period, you will receive information about 1) the number of ECU contributed by each of your group members, with the individual contributions being sorted in descending order, 2) the income from the project, and 3) your corresponding period-earnings.

Your final earnings

Your final earnings will be calculated by adding up your period-earnings in each of the 10 periods. The resulting sum will be converted to euros and paid out to you in cash, together with the show-up fee of 2.50 euros.

Before the experiment starts, you will have to answer some control questions to verify your understanding of the rules of the experiment.

Please remain quietly seated until the experiment starts. If you have any questions, please raise your hand now.

A.2.2 Additional Instructions for Treatment PC

Interaction with your group members in each period

In each period, you as well as the other three members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

Before making the 10 contribution decisions, one group member is given the opportunity to communicate with his/her fellow members (how communication is carried out is described below). In the following, we shall refer to the group member who can communicate with the others as the "communicator".

At the beginning of the experiment, one member of each group is randomly selected to be the "communicator". Every participant will be informed whether he or she is going to act as the "communicator" in an "Information Window".

Communication

During the communication stage (i.e., before period 1), the communicator can use his/her keyboard to type one message to the others.

The communicator is free to send the message (s)he likes, including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.

However, *there are two restrictions on the kind of messages that the communicator can send*:

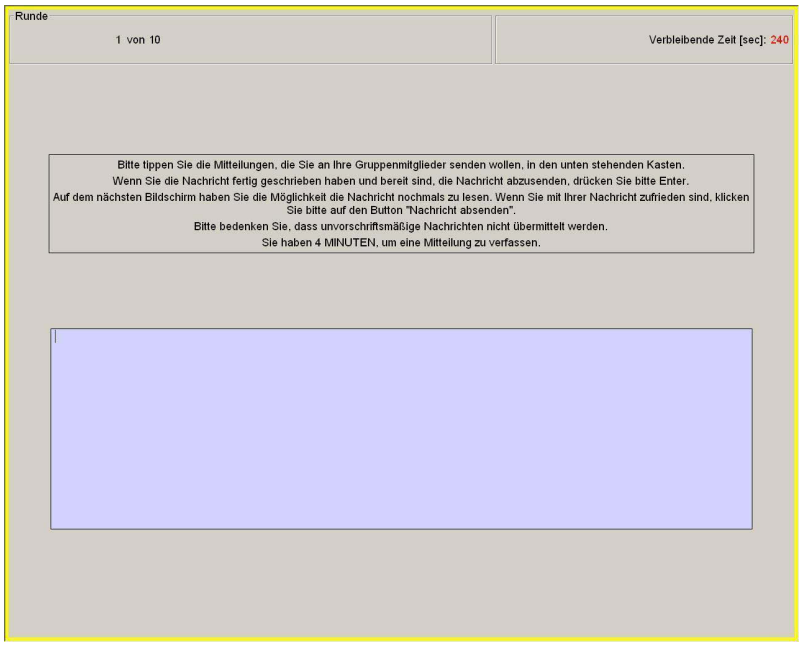
1. First, the communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. Second, there must be neither threats nor promises pertaining to anything that is to occur after the experiment.

To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper messages are not be delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

The communicator has 4 minutes to write his/her message, but (s)he is free

to send it ahead of time. A clock will inform the communicator of the remaining time.

The screen-shots that you will see in period 1 if you are the communicator in your group are shown below.



Please, remember to remain quiet during the whole experiment or the session will be terminated and all payments canceled.

A.2.3 Additional Instructions for Treatment CC

Interaction with your group members in each period

Each period consists of the following two stages:

1. One group member is given the opportunity to communicate with his/her fellow members (how communication is carried out is described below). In the following, we shall refer to the group member who can communicate with the others as the "communicator".
2. When the communication stage ends, the four group members decide simultaneously and privately on their own contributions.

At the beginning of the experiment, one member of each group is randomly selected to be the "communicator" for all 10 periods. Every participant will be informed whether he or she is going to act as the "communicator" in an "Information Window".

Communication

During the communication stage, the communicator can use his/her keyboard to type one message to the others.

The communicator is free to send the message (s)he likes, including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.

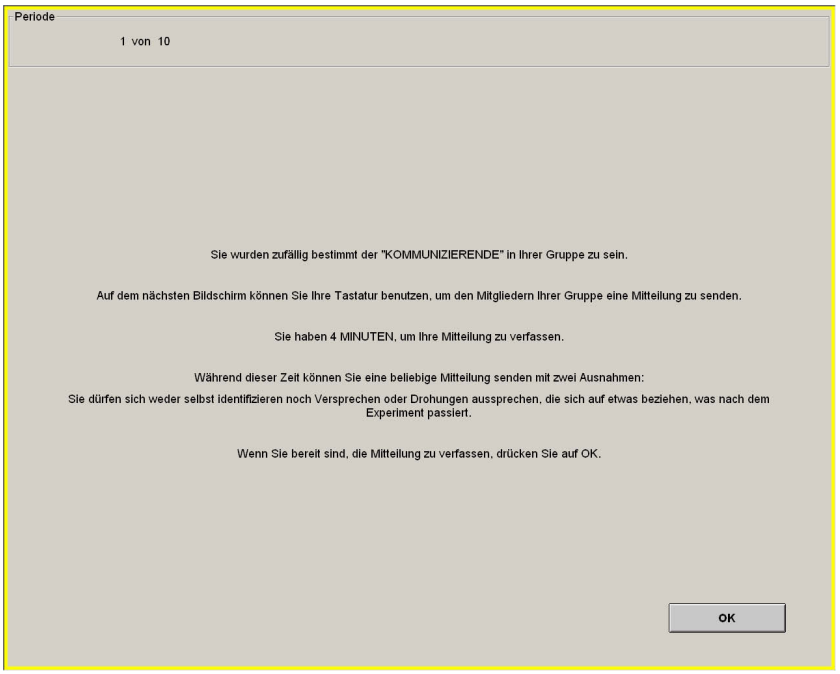
However, *there are two restrictions on the kind of messages that the communicator can send*:

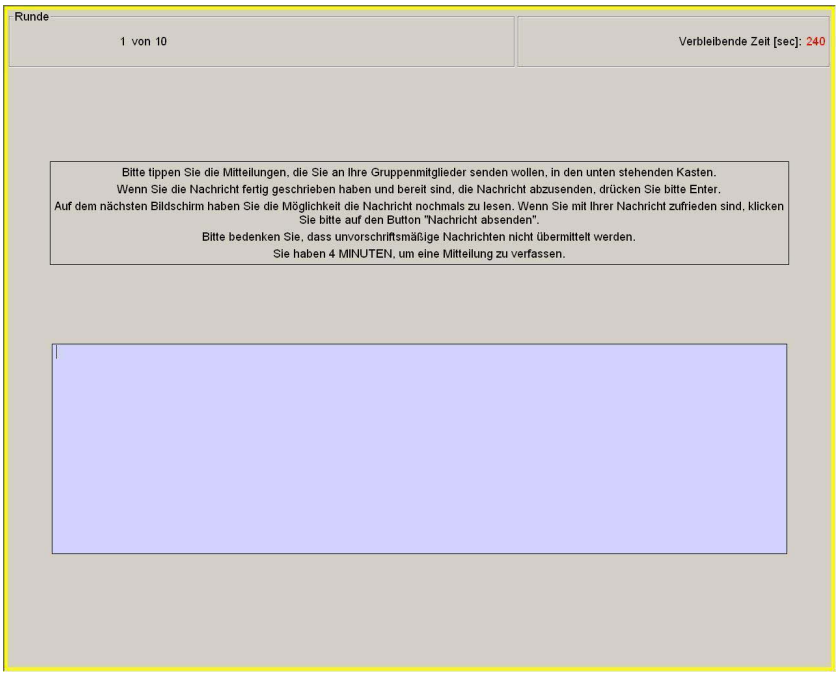
1. First, the communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. Second, there must be neither threats nor promises pertaining to anything that is to occur after the experiment.

To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper messages are not be delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

The communicator has 4 minutes to write his/her message, but (s)he is free to send it ahead of time. A clock will inform the communicator of the remaining time.

The screen-shots that you will see in period 1 if you are the communicator in your group are shown below.





In periods 2–10, if you are the communicator, you will see the following screen.

Please, remain quiet during the whole experiment or the session will be terminated and all payments canceled.

A.3 Instructions for One-Shot Interaction Treatments

This appendix reports the instructions (originally in German) for treatments B_1 and C. Those for treatment B_1 are displayed below in full length. They contain all parts which are common to both treatments. The instructions for treatment C can be obtained by inserting and replacing the appropriate paragraphs. The place holder *[for treatment C, replace the following paragraph]* indicates which paragraphs have to be replaced, where the replacement always has the same heading. The place holder *[for treatment C, insert paragraph <paragraph name> here]* prescribes where new paragraphs have to be inserted. Minor discrepancies between treatments are indicated in italics.

A.3.1 Instructions for Treatment B_1

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please remain silent and switch off your mobile.

You will receive 2.50 euro for showing up on time. Beyond this you can earn more money. In order to do this, please read these instructions carefully. The 2.50 euro show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., without the other participants knowing the extent of your earnings.

During the experiment, we shall not speak of euros but of ECUs (Experimental Currency Units). ECUs are converted to euros at the following exchange rate: 1 ECU = 0.40 euro.

It is strictly forbidden to speak to other participants. If you have any questions during the experiment please raise your hand.

Detailed information on the experiment

Group formation

You will be placed in a group of four players. You will never learn the identity of the other members of your group.

Decisions

The experiment consists of one period only. In this period you (as well as the other members of your group) receive an endowment of 25 ECUs. You have to decide **how many of these 25 ECUs you want to contribute to a project**. The ECUs contributed to the project yield income for you as well as for the other members of your group (you will learn more about the "income from the project" below). You can keep the ECUs that you do not contribute for yourself (they yield income just for you).

Period-earnings

More specifically, your period-earnings consist of two parts:

a) "Income from the project" = 0.4 $\times$ sum of all group members' contributions (in words, the income from the project is determined by multiplying the sum of the contributions of all group members by 0.4);

b) "ECUs you keep" = 25 $-$ your contribution to the project.

Thus, your period-earnings summarized in a formula are

Your period-earnings $=$	**Income from the project**	$+$	**ECU you keep**
	(0.4 $\times$ sum of group's contributions)	$+$	(25 $-$ your contribution)

Example:
Suppose that all four group members contribute 5 ECUs. Then both you and your group members receive an "income from the project" of 8 (= 0.4 $\times$ 20) ECUs. The "ECUs you keep" are 20 ($= 25 - 5$). Hence, your period-earnings are 8 + 20 = 28 ECUs.

[*for treatment C, replace the following paragraph]*

Interaction with your group members

You as well as the other three members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

[*for treatment C, insert paragraph Communication here]*

The information you receive at the end of the experiment

You will receive information about 1) the number of ECU contributed by each of your group members, with the individual contributions being sorted in descending order, 2) the income from the project, and 3) your corresponding period-earnings.

Your final payoff

At the end of the experiment, your period-earnings will be converted into euros and paid out to you in cash, together with the show-up fee of 2.50 euros.

Before the experiment starts, you will have to answer some control questions to verify your understanding of the rules of the experiment. Once everybody has answered all questions correctly, six practice periods will be played *[(only in treatment C:) which will only include the decision situation, but not the communication stage]*. During these six periods, you will not be matched with other persons in this room, but with a computer that will determine randomly the others' decisions. You will get no payment for these practice periods.

Please remain quietly seated until the experiment starts. If you have any questions, please raise your hand now.

A.3.2 Additional Instructions for Treatment C

Interaction with your group members

You as well as the other three members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

Before making your contribution decision, one group member is given the opportunity to communicate with his/her fellow members (how communication is carried out is described below). In the following, we shall refer to the group member who can communicate with the others as the "communicator".

At the beginning of the experiment, one member of each group is randomly selected to be the "communicator". Every participant will be informed whether he or she is going to act as the "communicator" in an "Information Window".

Communication

During the communication stage the communicator can use his/her keyboard to type one message to the others.

The communicator is free to send the message (s)he likes, including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.

However, *there are two restrictions on the kind of messages that the communicator can send*:

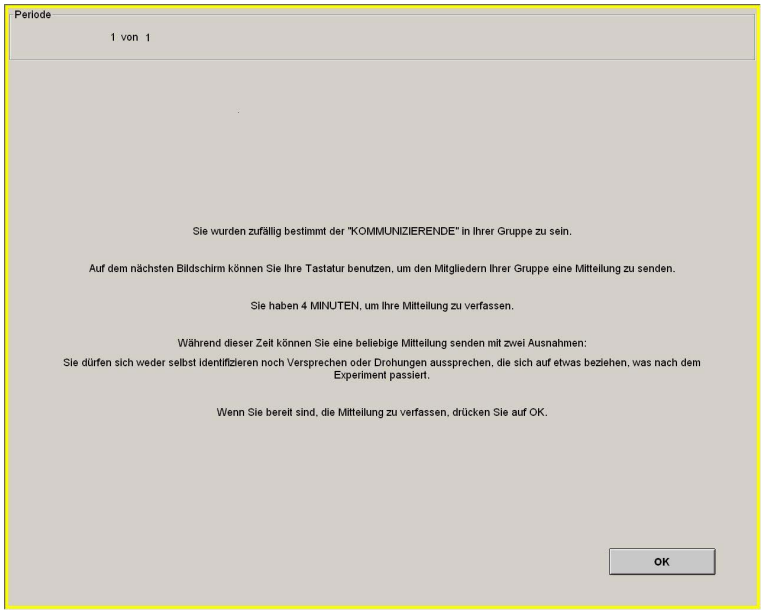
1. First, the communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. Second, there must be neither threats nor promises pertaining to anything that is to occur after the experiment.

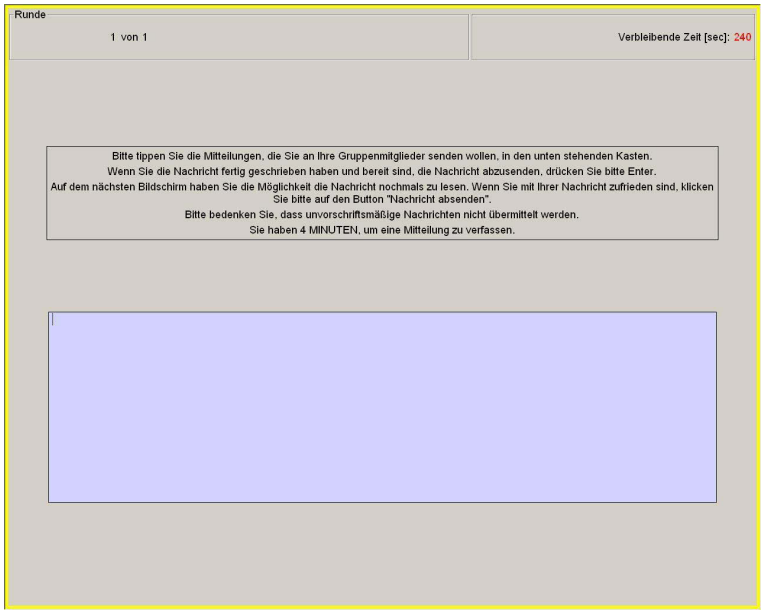
To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper messages are not be delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

The communicator has 4 minutes to write his/her message, but (s)he is free to send it ahead of time. A clock will inform the communicator of the remaining

time.

The screen-shots that you will see if you are the communicator in your group are shown below.





Please, remember to remain quiet during the whole experiment or the session will be terminated and all payments canceled.

APPENDIX B

APPENDIX TO CHAPTER 3

B.1 Robustness Checks

Table 3.2 in Chapter 3.3 reports on two random-effects Tobit regressions that model the individuals' relative contributions to the public good. The main result is that rich subjects tend to contribute significantly lower proportions of their endowments than poor subjects in treatment $B_{10,A}$ but not in treatment CC_A. Here we show that this result is robust to alternative specifications.

Tables B.1 and B.2 report on two specifications for each treatment, both of which involve a constant, the linear trend t, the endowment dummy $d_{endowment}$, and the average of the relative contributions of the group in the previous period. The second specification additionally involves the time trend squared. The results show that the endowment dummy is significant for treatment $B_{10,A}$ but not for treatment CC_A, irrespective of the specification.[1] The Bayesian information criterion favors specification (1) over specification (2) for treatment $B_{10,A}$ but advices the reverse ordering for treatment CC_A. Hence, our choice for Table 3.2.

Table B.3 provides a further robustness check. It reports the results from bootstrapped random-effects Tobit models with the same set of variables as the specifications in Table 3.2. All results remain robust.

[1] This result is robust to a specification that includes a dummy variable for the end game effect instead of the time trend squared. The result is also robust when we drop the average of the relative contributions of the group in the previous period. Likelihood ratio tests, however, advice its inclusion.

Table B.1: Treatment $B_{10,A}$: Random-Effects Tobit Regression – Robustness Check

	(1)	(2)
constant	0.352 (0.000)	0.299 (0.001)
t	−0.035 (0.000)	−0.007 (0.779)
t^2		−0.002 (0.286)
$d_{endowment}$	−0.168 (0.001)	−0.168 (0.001)
$\frac{\sum_{j=1}^{4} c_{j,t-1}/e_{j,t-1}}{4}$	0.807 (0.000)	0.780 (0.000)
St. dev.		
Random intercept	0.175 (0.000)	0.178 (0.000)
Residual	0.289 (0.000)	0.288 (0.000)
Wald test	199.89 (0.000)	199.38 (0.000)
BIC	552.7	557.9

Note: The dependent variable is $c_{i,t}/e_{i,t}$ (576 observations grouped by subject). The regression involves 91 left censored and 74 right censored observations. t stands for trend; $d_{endowment}$ equals 0 for the poor and 1 for the rich. Significance levels are reported in parentheses. BIC stands for Bayesian information criterion.

Table B.2: Treatment CC_A: Random-Effects Tobit Regression – Robustness Check

	(1)	(2)
constant	0.308 (0.170)	−0.461 (0.105)
t	−0.060 (0.000)	0.322 (0.000)
t^2		−0.032 (0.000)
$d_{endowment}$	−0.111 (0.455)	−0.111 (0.501)
$\frac{\sum_{j=1}^{4} c_{j,t-1}/e_{j,t-1}}{4}$	1.959 (0.000)	1.790 (0.000)
St. dev.		
Random intercept	0.465 (0.000)	0.536 (0.000)
Residual	0.643 (0.000)	0.604 (0.000)
Wald test	98.45 (0.000)	99.94 (0.000)
BIC	631.8	614.7

Note: The dependent variable is $c_{i,t}/e_{i,t}$ (576 observations grouped by subject). The regression involves 53 left censored and 401 right censored observations. t stands for trend; $d_{endowment}$ equals 0 for the poor and 1 for the rich. Significance levels are reported in parentheses. BIC stands for Bayesian information criterion.

Table B.3: Random-Effects Tobit Regression Results for Relative Contributions to the Public Good - BCa Bootstraps

Dep. variable: c_i/e_i Random intercepts: individual				
	$B_{10,A}$		CC_A	
	Coefficient	95% CI (BCa)	Coefficient	95% CI (BCa)
t	-0.035***	[-0.05; -0.02]	0.322**	[0.09; 0.66]
t^2			-0.032***	[-0.06; -0.01]
$\mathrm{d}_{\text{endowment}}$	-0.168***	[-0.25; -0.09]	-0.111	[-0.36; 0.08]
$\frac{\sum_{j=1}^{4} c_{j,t-1}/e_{j,t-1}}{4}$	0.807***	[0.70; 1.01]	1.790***	[1.16; 3.79]
Constant	0.352***	[0.23; 0.46]	-0.461	[-2.25; 0.35]
St. dev.				
Random intercept	0.175***	[0.12; 0.22]	0.536***	[0.28; 0.79]
Residual	0.289***	[0.26; 0.34]	0.604***	[0.46; 0.85]
Log likelihood	-257.3		-285.1	
BIC	552.7		614.7	

Note: The regressions are based on 576 observations, 64 subjects, and 16 groups. The $B_{10,A}$ (CC_A) regression involves 91 (53) left-censored and 74 (401) right-censored observations. t stands for trend; $d_{\text{endowment}}$ equals 0 for the poor and 1 for the rich. The bias corrected and accelerated (BCa) confidence intervals are based on non-parametric bootstraps with 500 replications. Sampling respects group composition. ***, **, * indicate significance at levels 1%, 5%, and 10%, based on the BCa confidence intervals. BIC stands for Baysian information criterion.

B.2 Instructions

This appendix reports the instructions (originally in German) for both treatments treatments $B_{10,A}$ and CC_A.[2] Those for treatment $B_{10,A}$ are displayed below in full length. They contain all parts which are common to both treatments. The instructions for treatment CC_A can be obtained by inserting and replacing the appropriate paragraphs. The place holder *[for treatment CC_A, replace the following paragraph]* indicates which paragraphs have to be replaced, where the replacement always has the same heading. The place holder *[for treatment CC_A, insert paragraph <paragraph name> here]* prescribes where new paragraphs have to be inserted.

B.2.1 Instructions for Treatment $\mathrm{B_{10,A}}$

INSTRUCTIONS

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please remain silent and switch off your mobile.

You will receive 2.50 euros for showing up on time. If you read these instructions carefully, you can make profitable decisions and earn more. The show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., without the other participants knowing the extent of your earnings.

During the experiment, we shall not speak of euros but of ECUs (Experimental Currency Units). ECUs are converted to euros at the following exchange rate: 1 ECU = 0.05 euro.

It is strictly forbidden to speak to other participants. If you have any questions during the experiment please raise your hand.

[2]Since treatments $B_{10,S}$ and CC_S correspond to treatments B_{10} and CC in Chapter 2, respectively, the instructions for these treatments can be found in Appendix A.2.

Detailed information on the experiment

Group formation

You will be placed in a group of four players. Group composition does *not* change during the experiment, i.e., you will be always interacting with the same participants. You will never learn the identity of the other members of your group.

Decisions

The experiment consists of 10 periods. Before the experiment starts, you will be randomly assigned to either of two types: type A or type B. Type A members receive an endowment of 30 ECUs at the beginning of each period. Type B members receive an endowment of 20 ECUs per period. Each group has two members of type A and two members of type B.

You have to decide **how much of your endowment you want to contribute to a project**. The ECUs contributed to the project yield income for you as well as for the other members of your group (you will learn more about the "income from the project" below). You can keep for yourself the ECUs that you do not contribute (they yield income just for you).

Period-earnings

More specifically, in every period your earnings consist of two parts:

a) "income from the project" = 0.4 $\times$ sum of all group members' contributions (in words, the income from the project is determined by multiplying the sum of the contributions of all group members by 0.4);

b) "ECUs you keep" = your endowment $-$ your contribution to the project.

Thus, your period-earnings summarized in a formula are

Your period-earnings	$=$	**Income from the project**	$+$	**ECU you keep**
		$(0.4 \times$ sum of group's contributions$)$	$+$	(your endowment $-$ your contribution)

Example:

Suppose that all four group members contribute 5 ECUs. Then both you and your

group members receive an "income from the project" of 8 (= 0.4 $\times$ 20) ECUs. The "ECUs you keep" are 25 ($= 30 - 5$) if you are of type A and 15 ($= 20 - 5$) if you are of type B. Hence, your period-earnings are 8 + 25 = 33 ECUs if you are of type A and 8 + 15 = 23 ECUs if you are of type B.

[*for treatment CC_A, replace the following paragraph]*

Interaction with your group members in each period

In each period, you as well as the other three members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

[*for treatment CC_A, insert paragraph Communication here]*

The information you receive at the end of each period

At the end of each period, you will receive information 1) about the number of ECU contributed by each of your group members, sorted in descending order with the respective endowments indicated next to the contributions, 2) about the income from the project, and 3) about your corresponding period-earnings.

Your final earnings

Your final earnings will be calculated by adding up your period-earnings in each of the 10 periods. The resulting sum will be converted to euros and paid out to you in cash, together with the show-up fee of 2.50 euros.

Before the experiment starts, you will have to answer some control questions to verify your understanding of the rules of the experiment.

Please remain quietly seated until the experiment starts. If you have any questions, please raise your hand now.

B.2.2 Additional Instructions for Treatment $\mathrm{CC_A}$

Interaction with your group members in each period

Each period consists of the following two stages:

1. One group member is given the opportunity to communicate with his/her fellow members (how communication is carried out is described below). In the following, we shall refer to the group member who can communicate with the others as the "communicator".
2. When the communication stage ends, the four group members decide simultaneously and privately on their own contributions.

At the beginning of the experiment, one member of each group is randomly selected to be the "communicator" for all 10 periods. The selected group member sees this in an "Information Window", which appears on his/her screen at the beginning of the experiment.

Communication

During the communication stage, the communicator can use his/her keyboard to type one message to the others.

The communicator is free to send the message (s)he likes, including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.

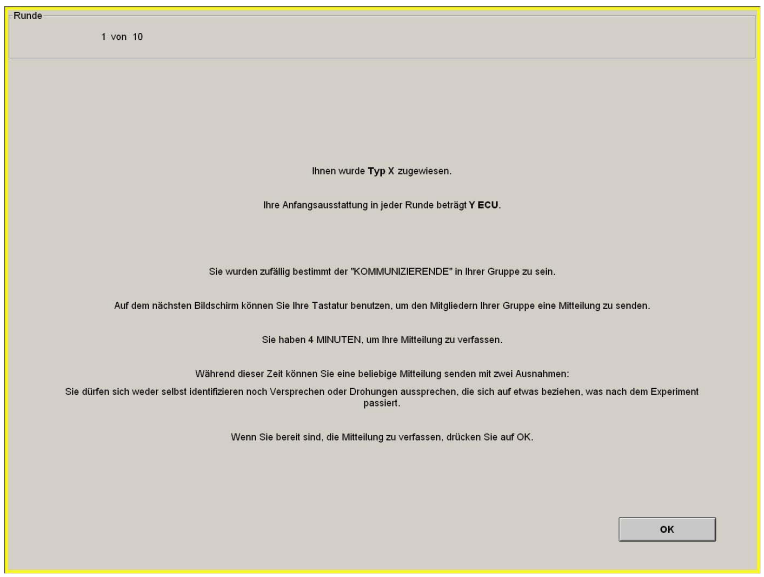
However, *there are two restrictions on the kind of messages that the communicator can send*.

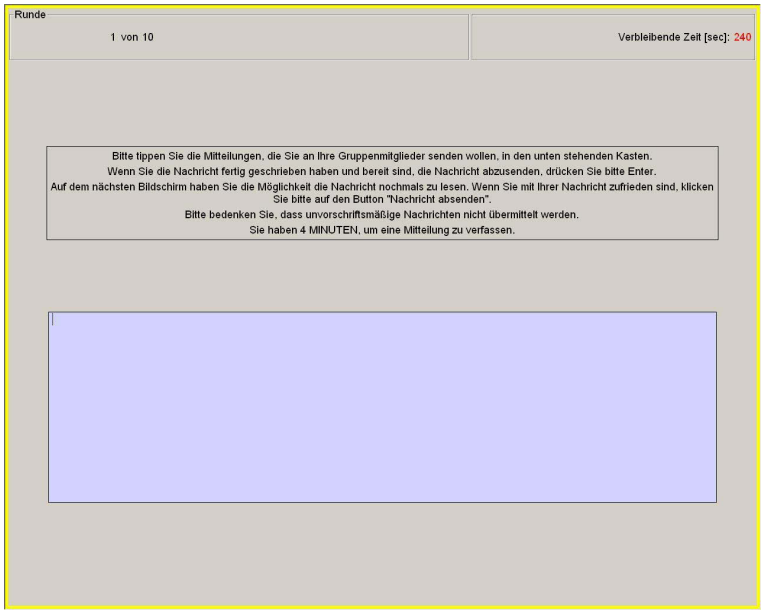
1. First, the communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. Second, there must be neither threats nor promises pertaining to anything that is to occur after the experiment.

To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper messages are not be delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

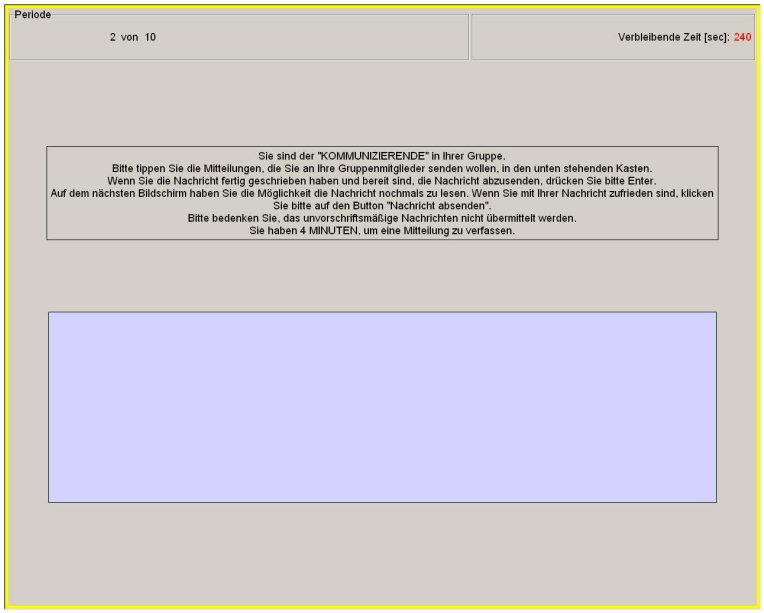
The communicator has 4 minutes to write his/her message, but (s)he is free to send it ahead of time. A clock will inform the communicator of the remaining time.

The screen-shots that you will see in period 1 if you are the communicator in your group are shown below, where the "X" replaces your type and the "Y" replaces your endowment.





In periods 2–10, if you are the communicator, you will see the following screen.



Please, remember to remain quiet during the whole experiment or the session will be terminated and all payments canceled.

APPENDIX C

APPENDIX TO CHAPTER 4

C.1 Post Experimental Questionnaire

For items 1 to 4 and item 9, participants were asked to state their degree of agreement ("not at all" to "very much") on a 7 point Likert scale. All other items asked for an integer input between 0 and 10. Belief elicitation was tailored with respect to the player type indicated in italics after items 5 to 8. Except for the first four items which measured group identification, every item appeared on a separate screen. Originally, all items were in German.

1. I feel committed to my group.
2. I am glad to be in my group.
3. I feel solidarity with my group.
4. It is pleasant to be in my group.
5. According to your estimation, what is the other group members' average contribution to the project? *(normal groups)*
6. According to your estimation, what did the two members of type B contribute on average to the project? *(high-benefit members)*
7. According to your estimation, what did the member of type A contribute to the project? *(low-benefit members in privileged groups)*
8. According to your estimation, what did the other member of type B contribute to the project? *(low-benefit members in privileged groups)*
9. The communicator's message was constructed as to maximize his own payoff. *(non-communicators)*

C.2 Categorization Methodology

Our categorization methodology follows Cooper and Kagel (2005) and Sutter and Strassmair (2009). The set of categories is based on those obtained for treatment C in Chapter 2. The new messages were screened in order to identify potentially distinct arguments which were not represented in the original set of categories. Since none were found, we adopt the previous set of categories and report it in Table 4.3. For an exact description of how the original categories where established, see Appendix A.1.

Two undergraduate research assistants separately coded all messages obtained from treatment RC. If one message contained the argument(s) specified by a category, then that category was assigned the value of 1 (otherwise, it was assigned the value of 0). The correlation coefficient between the assistants' codings was 0.64 and 0.85 for the sets of normal and privileged groups, respectively. Finally, the two coders gathered and discussed their individual assessments and arrived at a common coding. The result is reported in Table 4.4.

C.3 Instructions

This appendix reports the instructions (originally in German) for all treatments. Those for normal groups in the baseline treatment are displayed in full length below. They contain all parts which are common to all six treatments. The instructions for privileged groups and the treatments RC and IC can be obtained by inserting and replacing the appropriate paragraphs. The place holder *[for <treatment name>, replace the following paragraph]* indicates which paragraphs have to be replaced, where the replacement always has the same heading. The place holder *[for <treatment name>, insert paragraph <paragraph name> here]* prescribes where new paragraphs have to be inserted. Minor discrepancies between treatments are indicated in italics.

C.3.1 Instructions for Normal Groups - Baseline Treatment

INSTRUCTIONS

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please remain silent and switch off your mobile.

You will receive 2.50 euro for showing up on time. Beyond this you can earn more money. In order to do this, please read these instructions carefully. The 2.50 euro show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., without the other participants knowing the extent of your earnings. During the experiment, we shall not speak of euros but of ECUs (Experimental Currency Units). ECUs are converted to euros at the following exchange rate: 1 ECU = 0.20 euro.

It is strictly forbidden to speak to other participants. If you have any questions during the experiment please raise your hand.

Detailed information on the experiment

Group formation

You will be placed in a group of three players. You will never learn the identity of the other members of your group.

Decisions

The experiment consists of one period only. In this period, you (as well as the other members of your group) receive an endowment of 25 ECUs. You have to decide **how many of these 25 ECUs you want to contribute to a project**. The ECUs contributed to the project yield income for you as well as for the other members of your group (you will learn more about the "income from the project" below). You can keep the ECUs that you do not contribute for yourself.

[*for privileged groups, replace the following paragraph]*

Period-earnings

More specifically, your period-earnings consist of two parts:

a) "Income from the project" = 0.6 $\times$ sum of all group members' contributions (in words, the income from the project is determined by multiplying the sum of the contributions of all group members by 0.6);

b) "ECUs you keep" = 25 $-$ your contribution to the project.

Thus, your period-earnings summarized in a formula are

Your period-earnings	$=$	**Income from the project**	$+$	**ECU you keep**
		(0.6 $\times$ sum of group's contributions)	$+$	(10 $-$ your contribution)

Example:

Suppose that all three group members contribute 5 ECUs. Then both you and your group members receive an "income from the project" of 9 ECUs (= 0.6 $\times$ 15). The "ECUs you keep" are 20 ($= 25 - 5$). Hence, your period-earnings are 9 + 20 = 29 ECUs.

[*for treatments IC and RC, replace the following paragraph]*

Interaction with your group members

You as well as the other two members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

[*for treatments IC and RC, insert paragraph Communication here]*

The information you receive at the end of the experiment

You will receive information about 1) the number of ECU contributed by each of your group members, with the individual contributions being sorted in descending order, 2) the income from the project, and 3) your corresponding period-earnings.

Your final payoff

At the end of the experiment, your period-earnings will be converted into euros and paid out to you in cash, together with the show-up fee of 2.50 euros.

Before the experiment starts, you will have to answer some control questions to verify your understanding of the rules of the experiment. Once everybody has answered all questions correctly, four practice periods will be played *[(only for treatments IC and RC:), which will only include the decision situation, but not the communication stage]*. During these four practice periods, you will not be matched with other persons in this room, but with a computer that will determine the others' decisions randomly. You will get no payment for these practice periods.

Please remain quietly seated until the experiment starts. If you have any questions, please raise your hand now.

C.3.2 Additional Instructions for Normal Groups

Communication

During the communication stage the communicator can use his/her keyboard to type one message to the others. The communicator is free to send the message (s)he likes, *[(only for relevant communication:), including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.] [(Only for irrelevant communication:). Its content may e.g. be related to a current topic or may be intended to entertain the other group members.]*

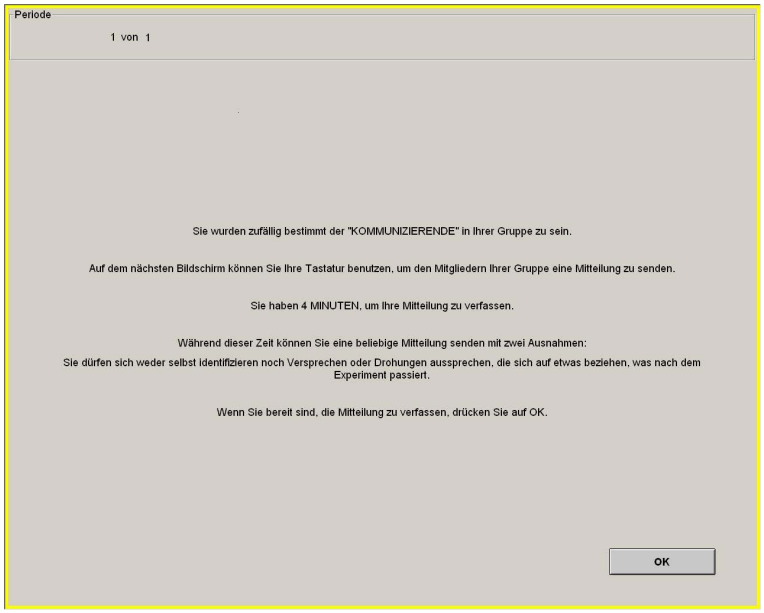
However, there are two *(only for irrelevant communication:) three* restrictions on the kind of messages that the communicator can send:

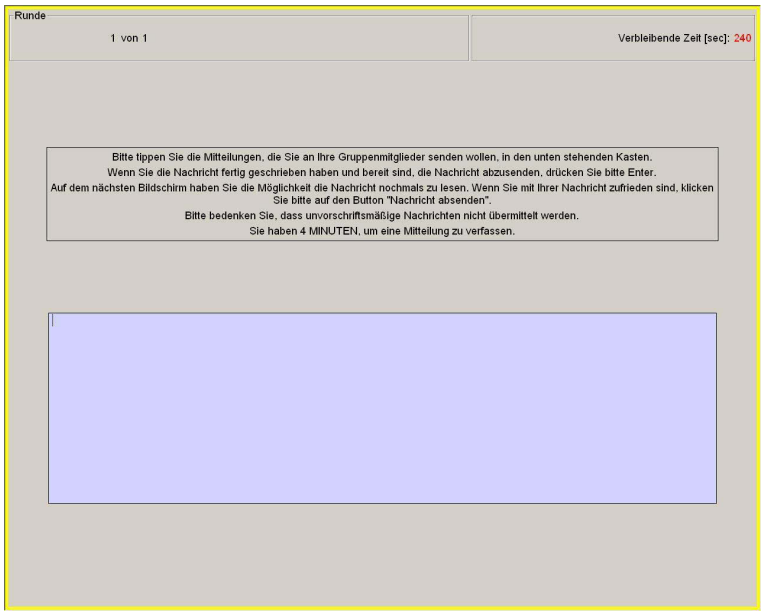
1. The communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. There must be neither threats nor promises pertaining to anything that is to occur after the experiment.
3. *[(Only for irrelevant communication:) Third, the communicator is not allowed to write about the upcoming decision situation. Thus, (s)he, e.g., must not indicate what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.]*

To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper messages are not delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

The communicator has 4 minutes to write his/her message, but (s)he is free to send it ahead of time. A clock will inform the communicator of the remaining time.

The screen-shots that you will see if you are the communicator in your group are shown below.





Please, remember to remain quiet during the whole experiment or the session will be terminated and all payments will be canceled.

C.3.3 Additional Instructions for Privileged Groups

Period-earnings

More specifically, your period-earnings consist of two parts:

a) "Income from the project" = **type-factor** $\times$ sum of all group members' contributions (in words, your income from the project is determined by multiplying the sum of the contributions of all group members by your **type-factor**);

b) "ECUs you keep" = 25 $-$ your contribution to the project.

Thus, your period-earnings summarized in a formula are

Your period-earnings $=$	**Income from the project**	$+$	**ECUs you keep**
	(**type-factor** $\times$ sum of group's contributions)	$+$	(25 $-$ your contribution)

Before the experiment starts, you will be randomly assigned to either of two types: type A or type B. Each group consists of one member of type A and two members of type B. The types differ exclusively in their "income from the project." More specifically:

- If you are of **type A**, your **type-factor** is **1.6**.
- If you are of **type B**, your **type-factor** is **0.6**.

[(Only for the baseline condition:) At the beginning of the experiment, one member of each group is randomly selected to be the "type A member." Every participant will be informed whether he or she is going to act as the "type A member" in an "Information Window."]

Example:
Suppose that all three group members contribute 5 ECUs. Then the "ECUs you keep" are 20 ($= 25 - 5$) for both you and for your group members. The "income from the project" is 24 ECU (= 1.6 $\times$ 15) if you are of type A and 9 ECU (= 0.6 $\times$ 15) if you are of type B. Hence, your period earnings are 24 + 20 = 44 ECUs if you are of type A and 9 + 20 = 29 ECUs if you are of type B.

Interaction with your group members

You as well as the other two members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project.

Before making your contribution decision, the type A member of every group is given the opportunity to communicate with his/her fellow members (how communication is carried out is described below). In the following, we shall refer to the type A member also as the "communicator."

At the beginning of the experiment, one member of each group is randomly selected to be the "type A member / communicator." Every participant will be informed whether he or she is going to act as the "type A member / communicator" in an "Information Window."

Communication

During the communication stage the communicator can use his/her keyboard to type one message to the others. The communicator is free to send the message (s)he likes, *[(only for relevant communication:), including what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.] [(Only for irrelevant communication:). Its content may e.g. be related to a current topic or may be intended to entertain the other group members.]*

However, there are two *(only for irrelevant communication:) three* restrictions on the kind of messages that the communicator can send:

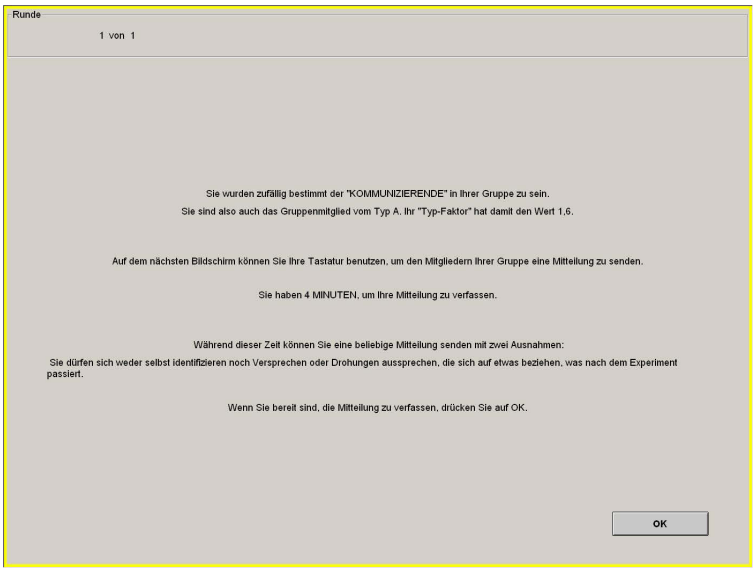
1. The communicator is not allowed to identify him/herself to the others. Thus, (s)he cannot reveal his/her real name, nicknames, or any other identifying feature such as gender, hair, or seat number.
2. There must be neither threats nor promises pertaining to anything that is to occur after the experiment.
3. *[(Only for irrelevant communication:) The communicator is not allowed to write about the upcoming decision situation. Thus, (s)he, e.g., must not indicate what (s)he thinks is the best approach to the experiment, what (s)he plans to do, or what (s)he would like the others to do.]*

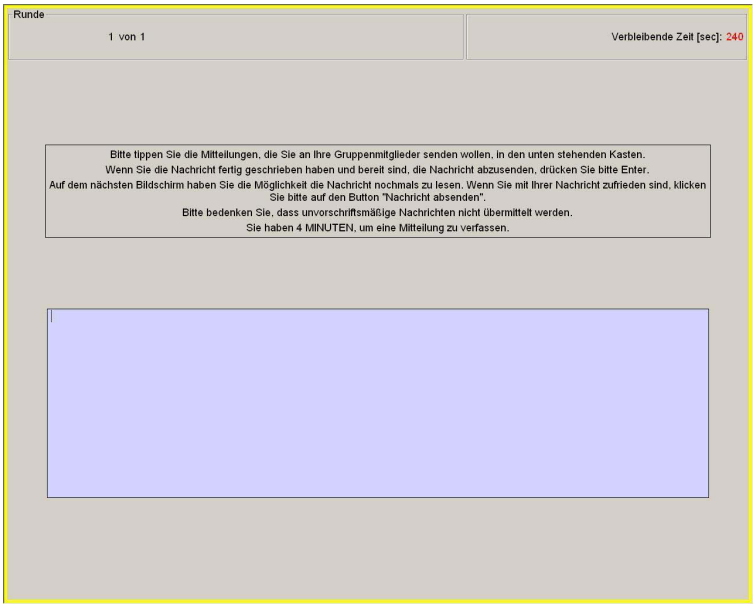
To enforce compliance with the above restrictions, all messages, before being sent, are checked by a monitor (a member of the experiment team). Improper

messages are not delivered. Instead, the sender receives a warning informing him/her of his/her misconduct.

The communicator has 4 minutes to write his/her message, but (s)he is free to send it ahead of time. A clock will inform the communicator of the remaining time.

The screen-shots that you will see if you are the communicator in your group are shown below.





Please, remember to remain quiet during the whole experiment or the session will be terminated and all payments will be canceled.

APPENDIX D

APPENDIX TO CHAPTER 5

D.1 Additional Empirical Results

Table D.1: Panel Tobit Regressions: Main and Interaction Effects - Robustness Check

Dep. variable: individual contributions Random intercept: group				
	(3)		**(4)**	
	Coefficient	95% CI (BCa)	Coefficient	95% CI (BCa)
Competition	2.029***	[0.63; 3.37]	1.300	[-0.26; 2.96]
Sequential	1.670***	[0.53; 2.78]	0.683	[-1.58; 2.97]
Comp*Sequential	–	–	1.461	[-1.12; 4.43]
Period	-0.214***	[-0.34; -0.11]	-0.214***	[-0.34; -0.11]
Age	-0.100**	[-0.20; -0.01]	-0.100**	[-0.20; -0.01]
Gender	-0.484	[-1.14; 0.06]	-0.479	[-1.14; 0.07]
Constant	6.147***	[3.66; 8.35]	6.608***	[4.27; 9.35]
St. dev. random intercept	3.104***	[2.66; 3.59]	3.087***	[2.67; 3.61]
St. dev. residual	3.290***	[2.94; 3.88]	3.290***	[2.94; 3.88]
Log likelihood	-4842.0		-4841.6	

Note: The regressions are based on 2310 observations, 231 individuals, and 77 groups. 448 (293) observations are left (right) censored. The biased corrected and accelerated (BCa) confidence intervals are based on a non-parametric bootstrap with 500 replications. Sampling respects group composition. ***, **, * indicate significance at levels 1%, 5%, and 10%, based on the BCa confidence intervals.

Table D.2: Linear Mixed Effects Regressions: Main and Interaction Effects - Robustness Check

Dep. variable: individual contributions Random intercepts: group, individual				
	(5)		**(6)**	
	Coefficient	95% CI (BCa)	Coefficient	95% CI (BCa)
Competition	1.379***	[0.42; 2.23]	1.098**	[0.03; 2.16]
Sequential	1.285***	[0.53; 2.03]	0.906	[-0.63; 2.35]
Comp*Sequential	–	–	0.561	[-1.15; 2.59]
Period	-0.154***	[-0.23; -0.09]	-0.154***	[-0.23; -0.09]
Age	-0.076**	[-0.13; -0.01]	-0.075**	[-0.13; -0.01]
Gender	-0.290*	[-0.60; 0.01]	-0.285*	[-0.60; 0.02]
Constant	6.011***	[4.37; 7.32]	6.181***	[4.63; 7.61]
St. dev. random intercepts				
Group intercept	2.044***	[0.61; 0.82]	2.040***	[0.61; 0.82]
Individual intercept	1.009	[-0.19; 0.08]	1.009	[-0.19; 0.08]
St. dev. residual	2.262***	[0.75; 0.90]	2.262***	[0.75; 0.90]
Log likelihood	-5375.3		-5375.1	

Note: The regressions are based on 2310 observations, 231 individuals, and 77 groups. The bias corrected and accelerated (BCa) confidence intervals are based on non-parametric bootstraps with 500 replications. Sampling respects group composition. ***, **, * indicate significance at levels 1%, 5%, and 10%, based on the BCa confidence intervals.

D.2 Instructions - Stage 1

This appendix reports the instructions (originally in German) for the first stage of the experiment. Those for treatment S1-SeqNC are displayed below in full length. They contain all parts of the instructions common to both treatments in this stage. The instructions for treatment S1-SeqC can be obtained by inserting and replacing the appropriate paragraphs. The placeholders *[for treatment S1-SeqC, insert paragraph < paragraph name > here]* and *[for treatment S1-SeqC, replace the following paragraph]* indicate which paragraphs have to be added or replaced, where the replacement always has the same heading. Minor discrepancies between treatments are indicated in italics.

D.2.1 Instructions for Treatment S1-SeqNC

INSTRUCTIONS

Welcome and thank you for participating in this experiment. Please remain silent and switch off your mobile!

You will receive 2.50´euro for showing up on time. Beyond this you can earn more money. In order to do this, please read these instructions carefully. The 2.50 euro show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., without the other participants knowing the extent of your earnings. During the experiment, we shall not speak of euros but of ECU (Experimental Currency Units). ECU are converted to euros at the following exchange rate: 1 ECU = 0.80 euro.

The experiment consists of two parts. Some features of the experiment may change from the first to the second part. The instructions for the first part follow on this page. The instructions for the second part will be distributed after all participants have completed the first part.

It is strictly forbidden to speak to other participants. If you have any questions during the experiment please raise your hand.

DETAILED INFORMATION ON THE FIRST PART

You will first learn about the basic decision situation. The description about the experiment in the first part follows afterwards.

The Basic Decision Situation

Group formation

You will be placed in a group of three players. You will never learn the identity of the other members of your group. Every group will be identified by an individual color.

Decisions

You (as well as the other members of your group) receive an endowment of 10 ECU. You have to decide **how many of these 10 ECU you want to contribute to a project**. The ECU contributed to the group project yield income for you as well as for the other members of your group (you will learn more about the "income from the project" below). You can keep the ECU that you do not contribute for yourself (they yield income just for you).

[for treatments S1-SeqC, insert paragraph < Interaction with another group > here]

[for treatments S1-SeqC, replace the following paragraph]

Period earnings

More specifically, in every period your earnings consist of two parts:

a) "Income from the project" = 0.5 $\times$ sum of all group members' contributions (in words, the income from the project is determined by multiplying the sum of the contributions of all group members by 0.5);

b) "ECU you keep" = 10 $-$ your contribution to the project.

Thus, your period earnings summarized in a formula are

Your period earnings	$=$	**Income from the project**	$+$	**ECU you keep**
		$(0.5 \times$ sum of group's contributions$)$	$+$	$(10 -$ your contribution$)$

Example:
Suppose that all three group members contribute 5 ECU. Then both you and your group members receive an "income from the project" of 7.5 (= 0.5 $\times$ 15) ECU. The "ECU you keep" are 5 ($= 10 - 5$). Hence, your period earnings are 7.5 + 5 = 12.5 ECU.

The Experiment In The First Part

Interaction with your group members

This part of the experiment consists of one period only. This period entails the following two stages:

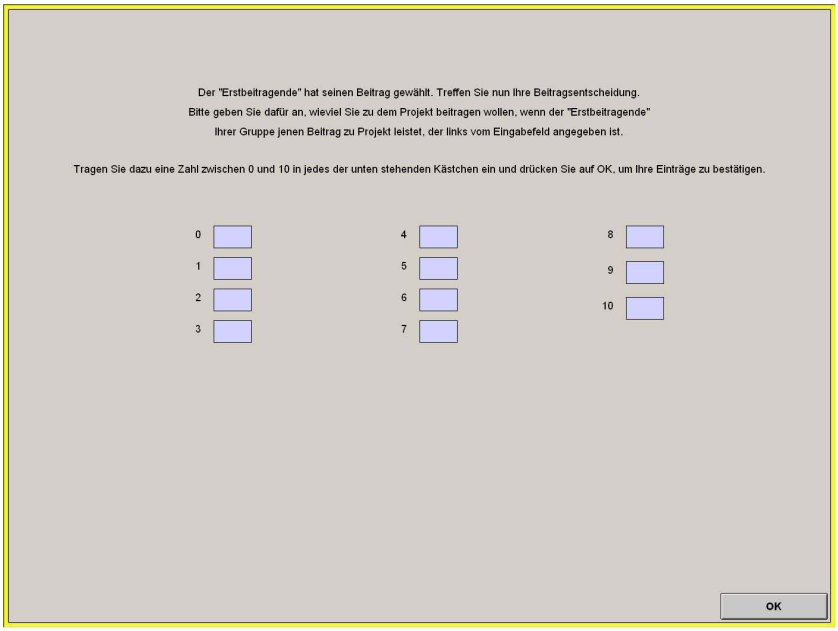
1. One group member decides prior to the others on his/her own contribution. In the following, we shall refer to the group member who decides first as the "early contributor."
2. Without learning the "early contributor's" choice, the other two group members decide simultaneously and privately on their own contributions. You will learn about the format of these decisions below.

At the beginning of the first part of the experiment, one member of each group is randomly selected to be the "early contributor." Every participant will be informed whether he or she is going to act as the "early contributor" in an "Information Window."

How you decide on your contribution

If you are the "early contributor," you enter your contribution in the following screen. You can insert any integer number from 0 to 10.

If you are not the "early contributor," you are going to be asked to indicate your contribution for every possible contribution of the "early contributor." The screen on which you will make your decisions is displayed below.



In each of the 11 boxes you have to indicate how many ECU you wish to contribute, conditional on the "early contributor's" contribution printed on the left of each box. In each box you can insert any integer from 0 to 10. Please bear in mind that the "early contributor" already made his/her decision, which can

not be revised. His/her choice determines which of your decisions will actually count. However, since you do not know his/her choice when making your decisions you will have to think carefully about all your decisions because all can become relevant to your earnings. The following example should clarify this.

Suppose that the "early contributor" decided to contribute 5 ECU to the project. Suppose furthermore that you decided on your contributions as displayed in the table below.

"early contributor's" decision	0	1	2	3	4	5	6	7	8	9	10
your contribution	0	10	0	7	8	8	0	3	2	8	0

Suppose furthermore, that the decisions of the third group member are identical to yours. The decision that counts for both you and the third group member is the one for the 5 ECU contribution of the "early contributor." I.e. you both contribute 8 ECU to the project. The sum of contributions thus equals $5 + 8 + 8 = 21$ ECU.

The information you receive

You will receive no information about any decision at the end of the first part of the experiment. Only when the second part of the experiment is finished you will be informed about the choices from the first part. This information includes (1) the "early contributor's" decision, (2) the corresponding decisions of the two other group members, (3) the income from the project, and *[(in treatment S1-SeqC) (4) the total contributions in your group and the group your's is paired with, and (5)]* (4) your resulting period earnings.

Additional information on the overall experiment

Your final payoff

Your final payoff will be based on only one of the two parts of the experiment. The payoff relevant part will be randomly selected by the flip of a fair coin at the end of the experiment (i.e., after everyone has finished the second part). The outcome of this coin flip will be decisive for everyone. If the first part of the experiment will be selected, you are going to be payed your period earnings in

this part in addition to the 2.50 euro show-up fee. The coin flip is going to be conducted by one of the participants of the experiment. To select the participant, one experimenter will draw a ball from an urn containing as many balls as there are participants in the experiment.

Before the experiment starts, we ask you to answer some control questions, in order to assure that all participants completely and correctly understood the rules of the experiment. Once everybody has answered all questions correctly, six practice periods will be played. During these six periods, you will not be matched with other persons in this room, but with a computer that will determine randomly the others' decisions. You will get no payment for these periods.

Please remain quietly seated until the experiment starts. If you have any questions, please raise your hand now. Please click on OK if you finished reading the instructions.

D.2.2 Additional Instructions for Treatment S1-SeqC

Interaction with another group

Your group will be randomly paired with another group of three. After the contribution decisions, the total (i.e., the sum of) contributions to the project of your group will be compared with the total contributions to the project of the other group. The group with the higher total contributions (or the "winning" group) receives a transfer from the group with the lower total contributions (or the "losing" group). The "transfer you receive or pay" depends on the difference in total contributions between the two groups and each group member will receive or pay an equal share of the transfer. You will learn more about the "transfer you receive or pay" below.

Period earnings

More specifically, in every period your earnings consist of three parts:

a) "Income from the project" = 0.4 $\times$ sum of all group members' contributions (in words, the income from the project is determined by multiplying the sum of the contributions of all group members by 0.4);

b) "ECU you keep" = 10 $-$ your contribution to the project;

c) "Transfer you receive or pay" = 0.1 $\times$ difference in total contributions between your group and the other group.

Thus, **if you are a member of the winning group**, your period earnings summarized in a formula are

Your period earnings	$=$	**Income from the project**	$+$	**ECU you keep**
		(0.4 $\times$ sum of group's contributions)	$+$	(10 $-$ your contribution)
	$+$	**Transfer you receive**		
		(0.1 $\times$ difference in total contributions)		

If you are a member of the losing group, your period earnings summarized in a formula are

Your period earnings	$=$	**Income from the project**	$+$	**ECU you keep**
		$(0.4 \times$ sum of group's contributions$)$	$+$	$(10 -$ your contribution$)$
	$-$	**Transfer you pay**		
		$(0.1 \times$ difference in total contributions$)$		

Example:

Suppose that all three members of your group contribute 5 ECU and all three members of the other group contribute 0 ECU. Then your group's total contributions are 3 $\times$ 5 ECU = 15 ECU. The other group's total contributions are 3 $\times$ 0 ECU = 0 ECU. This means that your group receives the transfer and the other group pays the transfer. The "transfer you receive" is 1.5 ($= 0.1 \times (15 - 0)$) ECU. The "income from the project" equals 6 (= 0.4 $\times$ 15) ECU. The "ECU you keep" are 5 ($= 10 - 5$). Hence, your period earnings are 6 + 5 + 1.5 = 12.5 ECU.

D.3 Instructions - Stage 2

This appendix reports the instructions (originally in German) for the second stage of the experiment. Those for treatment S2-SimNC are displayed below in full length. They contain all parts which are common to all four treatments in this stage. The instructions for the other treatments can be obtained by inserting and replacing the appropriate paragraphs. The place holder *[for treatment <treatment name>, replace the following paragraph]* indicates which paragraphs have to be replaced, where the replacement always has the same heading. The place holder *[for treatment <treatment name>, insert paragraph <paragraph name> here]* prescribes where new paragraphs have to be inserted. Minor discrepancies between treatments are indicated in italics.

D.3.1 Instructions for Treatment S2-SimNC

DETAILED INFORMATION ON THE SECOND PART

The Basic Decision Situation

In this second part you will face the same basic decision situation as in the first part of the experiment.

Group formation

The group composition is the same as in the first part of the experiment. I.e. you are again interacting with the same group members. *[(in S2-SimC and S2-SeqC:) Also, the group yours is interacting with is the same as in the first part of the experiment.]*

The Experiment In The Second Part

[*for treatments S2-SeqNC and S2-SeqC, replace the following paragraph]*

Interaction with your group members in each period

This part of the experiment consists of 10 periods. At the beginning of every period, each group member receives an endowment of 10 ECU. In each period,

you as well as the other two members of your group decide simultaneously and privately about the amount of ECUs you want to contribute to the project. The screen on which you will make your decisions is displayed below. You can insert any integer number from 0 to 10.

[*for treatments S2-SeqNC and S2-SeqC, insert paragraph <How you decide on your contribution> here]*

[*for treatments S2-SeqNC and S2-SeqC, replace the following paragraph]*

Your guess with respect to the own group

In every period, besides making your contribution decision, you have to make a guess. The target is to guess the average contribution of the other two members of your group (rounded to the next integer, 0.5 is rounded up). You will be paid for the accuracy of your guesses as follows:

- If your guess is the same as the target, you earn 3 ECU.
- If your guess deviates by 1 ECU from the target, you earn 2 ECU.
- If your guess deviates by 2 ECU or more, you earn nothing.

[*for treatments S2-SimC and S2-SeqC, insert paragraph <Your guess with respect to the other group> here]*

The information you receive after each period

After each period you will receive information about (1) the number of ECU contributed by each of your group members *[(in treatment S2-SimC and S2-SeqC) (1) the "early contributor's" decision, (2) the corresponding decisions of the other two group members]* being sorted in descending order, (2) the income from the project, *[(in treatment S2-SimC and S2-SeqC) (4) the total contributions in your group and the group your's is paired with,]* (3) your resulting period-earnings, and (4) the earnings for the accuracy of your guess.

Your final payoff

If the second part of the experiment is selected for payment, you are going to be paid according to one randomly selected period. For this period you will receive the sum of your period earnings and the payoff for the accuracy of your expectation *[(in treatments S2-SimC and S2-SeqC) both your expectations]*. In order to determine which period is payoff relevant, the randomly selected participant will draw a ball from an urn which contains 10 balls, numbered from 1 to 10. The draw will be decisive for everyone.

Please remain quietly seated until the experiment starts. If you have any questions, please raise your hand now. Please click on OK if you finished reading the instructions.

D.3.2 Additional Instructions for Treatments S2-SeqNC and S2-SeqC

Interaction with your group members in every period

This part of the experiment consists of 10 periods. At the beginning of every period, each group member receives an endowment of 10 ECU. Each period consists of the same two stages as in the first part of the experiment. The positions within each group are the same as in the first part of the experiment. In particular, if you were the "early contributor" in the first part, you are going to be the "early contributor" in the second part as well.

How you decide on your contribution

If you are the "early contributor," you enter your contribution in the following screen. You can insert any integer number from 0 to 10.

If you are not the "early contributor," you are not going to be asked to indicate your contribution for every possible contribution of the "early contributor" like in the first part of the experiment. Instead, you are informed about the "early contributor's" decision. Afterwards you can choose your own contribution. The screen on which you will make your decisions is displayed below, where the "X"

is the placeholder for the "early contributor's" choice. You can insert any integer number from 0 to 10.

Your guess with respect to the own group

In every period, besides making your contribution decision, you have to make a guess about the following target.

- If you are the "early contributor," the target is to guess the average contribution of the other two group members (rounded to the next integer, 0.5 is rounded up).
- If you are not the "early contributor," the target is to guess the contribution of the other group member who is in the same position as you.

You will be paid for the accuracy of your guesses as follows:

- If your guess is the same as the target, you earn 3 ECU.
- If your guess deviates by 1 ECU from the target, you earn 2 ECU.
- If your guess deviates by 2 ECU or more, you earn nothing.

D.3.3 Additional Instructions for Treatments S2-SimC and S2-SeqC

Your guess with respect to the other group

In every period you also have to guess the average contribution of the group yours is compared with (rounded to the next integer). You will be paid for the accuracy of your guesses as follows:

- If your guess is the same as the other group's average contribution, you earn 3 ECU.
- If your guess deviates by 1 ECU from the other group's average contribution, you earn 2 ECU.
- If your guess deviates by 2 ECU or more, you earn nothing.

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Erklärung gemäß § 4 Abs. 1 Pkt. 3 (PromO)

Hiermit erkläre ich,

1. dass mir die geltende Promotionsordnung bekannt ist;
2. dass ich die Dissertation selbst angefertigt, keine Textabschnitte eines Dritten oder eigener Prüfungsarbeiten ohne Kennzeichnung übernommen und alle von mir benutzten Hilfsmittel, persönliche Mitteilungen und Quellen in meiner Arbeit angegeben habe;
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Jena, den 22.01.2012 .....

(Johannes Weisser)

Liste der Veröffentlichungen, Arbeitspapiere und wissenschaftlichen Vorträge

Wissenschaftliche Veröffentlichungen

Nunnenkamp, P. Weingarth J., and Weisser, J., 2009. Is NGO aid not so different after all? Comparing the allocation of Swiss aid by private and official donors, *European Journal of Political Economy*, 25 (4), 422–438

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IAREP / SABE / ICABEEP, 2010, Cologne, 5.09.-08.09.2010, Präsentation: "Leading by words: A voluntary contribution experiment with one-way communication."

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